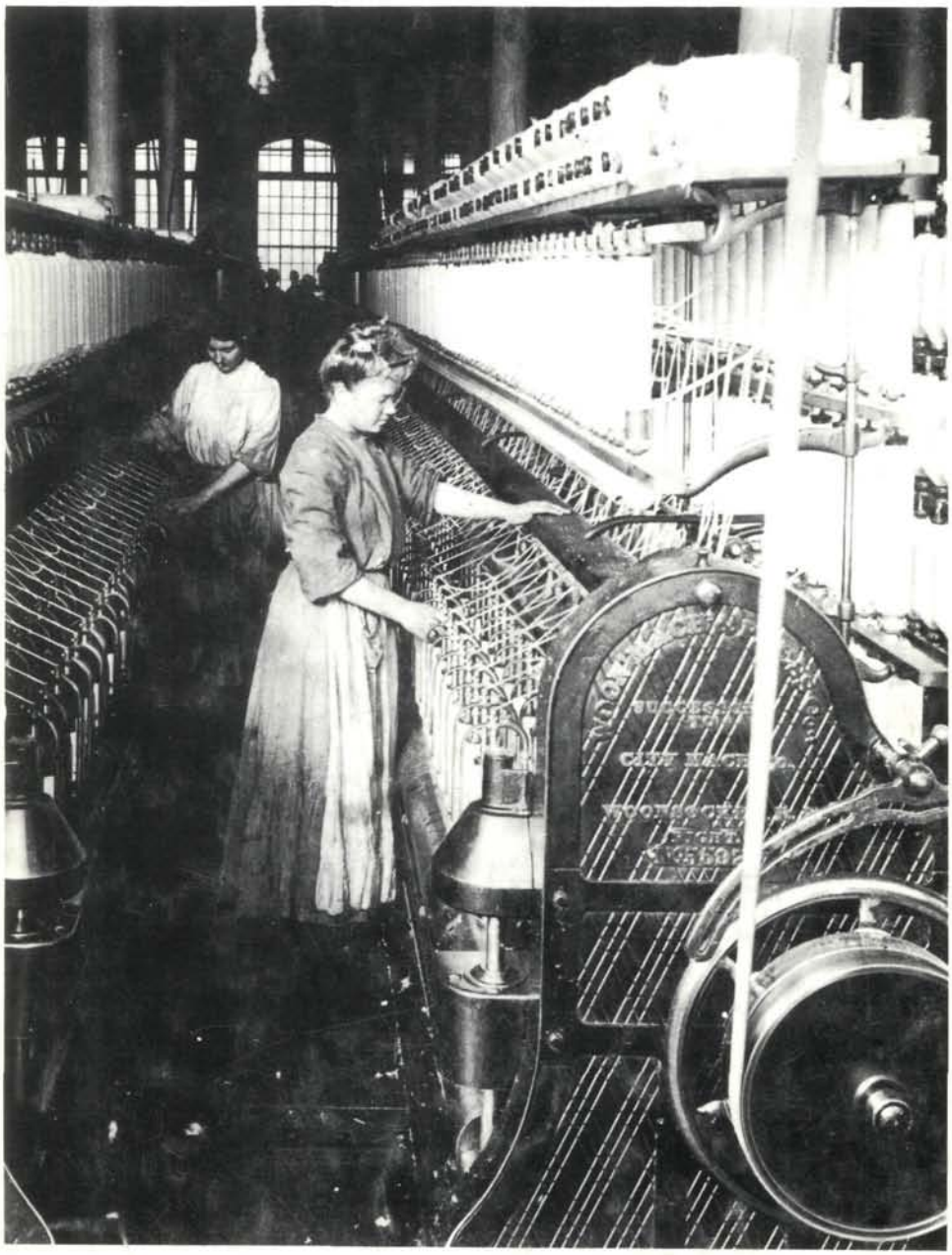
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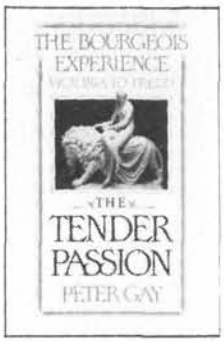
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In This Issue

Social history, labor history, southern history, and oral history are all features of our lead article. JAMES LELOUDIS, JACQUELYN DOWD HALL, and ROBERT KORSTAD describe the evolution of the southern textile industry and, in particular, life in the mill village as it emerged before World War I. They demonstrate that high rates of labor mobility, technological shortcomings at the mills, and the integration of lower management with the community constrained the seemingly all-powerful factory owners. By the time the owners sought to extend their control and to lower wages by technical innovation and indoctrination of workers—efforts that were aided by the labor surplus following the war—the people of the mill villages had built a sense of solidarity and transformed shared values into a foundation for collective action. The workers organized to defend their jobs and way of life in the years just after the war, again from 1927 to 1930, and finally in the General Strike of 1934. The defeat of this strike and subsequent failure of the CIO to break into the nonunion South brought a new era that saw the dismantling of the mill villages and the unopposed rationalization of the production process. The workers tell much of the story themselves, thanks to the files of the Oral History Program at the University of North Carolina.

The Somali coast in the nineteenth century can serve as a laboratory of history. A clan society of subsistence herders, gum gatherers, and fishermen survives through good years and bad by effectively exploiting a variable land that is poor in resources. In good years the people build a surplus; in drought years they conserve resources and suffer little. Add to this homeostatic system the loot of ships regularly wrecked along the coast, plus a nearby market at the British waystation of Aden across the gulf, and a political and economic transformation results. WAYNE DURRILL explains how one clan used its monopoly over the shipwreck loot and the patronage it generated to create a regional sultanate, which on its own initiative and in cooperation with local merchants, well before the advent of colonialism, propelled the peoples of the Horn into the world economy. The disruption of the ecological balance greatly increased the vulnerability of Somali pastoralists to poverty and drought. Much has been written lately about the origins of the East African famine in the dislocations wrought by colonialism. Durrill's article makes clear that this dislocation began earlier in Somalia and at a point of tangential contact with the world economic system.

In an article on urban segregation in West Africa after 1900, published in the June 1985 issue of the *AHR*, Philip Curtin noted that the policy rested on a half-century of medical doctrine and practice in India. JOHN CELL tests this assumption using evidence in medical textbooks of the period and annual reports of the Army Medical Department and the Sanitary Department of the Government of India. He finds that Anglo-Indian medical opinion did not advocate segregation as a measure to prevent the spread of tropical disease but held that the only approach with a chance of effectiveness was an attack on disease in the Indian population at large. The British justified the segregation they practiced in India on grounds of psychology and security, not prevention of disease. Lugard and others who promoted sanitary segregation in Africa based their policy on the prestige of science and often cited Indian precedents—and scholars came to accept these assertions. But the record does not support the claims. Apart from its historiographical importance, Cell's article sheds light on the responses of the Indian medical establishment to tropical diseases at a time that the germ theory was struggling for acceptance.

Critics of psychohistory have noted the dependence of its practitioners on an ahistorical method, and they have argued that this dependence disqualifies their work and compels us to abandon efforts to understand the psychological dimensions of the past. Much of this criticism is justified, writes THOMAS KOHUT, but the conclusion is not. Psychohistory need not rely on the psychohistorical method. Historians and practicing psychoanalysts approach their human subjects in the same way, and the two disciplines share a common method of understanding and explanation. Scholars can therefore employ traditional historical methods to comprehend the emotions and thought processes of their subjects. According to Kohut, the psychohistorical method should be abandoned—not psychohistory.

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JACQUELYN DOWD HALL, ROBERT KORSTAD, and JAMES LELOUDIS are working on a book-length study of southern textile workers. Hall received her Ph.D. from Columbia University and won the Bancroft Dissertation Award. Currently she is director of the Southern Oral History Program and associate professor at the University of North Carolina, Chapel Hill. She is the author of *Revolt Against Chivalry: Jessie Daniel Ames and the Women's Campaign Against Lynching* (1979), which won the Francis B. Simpkins and Lillian Smith awards, and of articles in Carol Berkin and Mary Beth Norton's *The Women of America: A Collection of Original Essays* (1979) and Sharon Thompson, Ann Snitow, and Christine Stansell's *Powers of Desire: The Politics of Sexuality* (1983). An article on women and labor militancy in Southern Appalachia is forthcoming in the *Journal of American History*. Robert Korstad and James Leloudis are Ph.D. candidates at the University of North Carolina, Chapel Hill. Korstad received his master's degree in economics from the New School for Social Research and is now completing a dissertation on tobacco manufacturing workers in Winston-Salem, North Carolina. He spent the last year as a Smithsonian Fellow at the National Museum of American History. Leloudis received his master's degree in history from Northwestern University and is working on a study of economic development and school reform in North Carolina between 1880 and 1930. His essay on southern education in the *Journal of American History* won the 1982 Louis Pelzer Award from the Organization of American Historians.

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Cotton Mill People: Work, Community, and Protest in the Textile South, 1880–1940

JACQUELYN DOWD HALL, ROBERT KORSTAD,
and
JAMES LELOUDIS

TEXTILE MILLS BUILT THE NEW SOUTH. Beginning in the 1880s, business and professional men tied their hopes for prosperity to the whirring of spindles and the beating of looms. Small-town boosterism supplied the rhetoric of the mill-building campaign, but the impoverishment of farmers was industrialization's driving force. The post–Civil War rise of sharecropping, tenantry, and the crop lien ensnared freedmen, then eroded yeoman society. Farmers of both races fought for survival by clinging to subsistence strategies and habits of sharing even as they planted cash crops and succumbed to tenantry. Meanwhile, merchants who had accummulated capital through the crop lien invested in cotton mills. As the industry took off in an era of intensifying segregation, blacks were relegated to the land, and white farmers turned to yet another strategy for coping with economic change. They had sold their cotton to the merchant; now they supplied him with the human commodity needed to run his mills. This homegrown industry was soon attracting outside capital and underselling northern competitors. By the end of the Great Depression, the Southeast replaced New England as the world's leading producer of cotton cloth, and the industrializing Piedmont replaced the rural Coastal Plain as pacesetter for the region.[1]

Earlier versions of this essay were presented at the Fourth Southern Labor Studies Conference, held in Atlanta, Georgia, September 30–October 2, 1982, and the Future of American Labor History Conference, held at Northern Illinois University, DeKalb, Illinois, October 10–12, 1984. Research was funded by a grant from the National Endowment for the Humanities, a William F. Sullivan Research Fellowship from the Museum of American Textile History, and a University Research Council Grant from the College of Arts and Sciences, University of North Carolina, Chapel Hill. Chris Daly, Lu Ann Jones, and Mary Murphy are our coauthors on a larger study of southern textile workers; we owe a special debt to their ideas and editorial skills. James Barrett, Craig Calhoun, Leon Fink, Susan Levine, and William Leuchtenburg offered helpful suggestions. Sydney Nathans read the essay at a critical juncture, and we have benefited constantly from his wisdom and support.

[1] C. Vann Woodward's work remains the essential guide to this period. See his *Origins of the New South, 1877–1913* (Baton Rouge, La., 1951). For a survey of the literature on the capitalist reorganization of southern agriculture, see Harold D. Woodman, "Sequel to Slavery: The New History Views the Postbellum South," *Journal of Southern History*, 43 (1977): 523–54. More recent works of particular relevance include Ronald D Eller, *Miners, Millhands, and Mountaineers: Industrialization of the Appalachian South, 1880–1930* (Knoxville, Tenn., 1982); Lawrence Goodwyn, *Democratic Promise: The Populist Moment in America* (New York, 1976); and Steven Hahn, *The Roots of Southern Populism: Yeoman*

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Despite the lasting imprint of textile manufacturing on regional development and labor relations, we have no modern survey of the industry's evolution. Nor has the outpouring of research on working-class history been much concerned with factory workers in the New South. To be sure, recent studies have uncovered sporadic, and sometimes violent, contention over the shape of the industrial South. But those findings have done little to shake the prevailing wisdom: The South's mill villages supposedly bred a "social type" compounded of irrationality, individualism, and fatalism. Unable to unite in their own interests, textile workers remained "silent, incoherent, with no agency to express their needs."[2]

We have reached different conclusions. Our research began with a collaborative oral history project aimed at discovering how working people made sense of their own experience. We did not view memory as a direct window on the past. But we did presume the moral and intellectual value of listening to those who lacked access to power and, thus, the means of affecting historical debate. Our effort was repaid in two major ways. Oral autobiographies dissolved static images, replacing them with portrayals of mill village culture drawn by the men and women who helped create it. Workers' narratives also steered us away from psychological interpretations and toward patterns of resistance, cultural creativity, and structural evolution. Later we turned to the trade press, particularly the *Southern Textile Bulletin*. Published by David Clark in Charlotte, North Carolina, the *Bulletin* spoke for factory owners at the cutting edge of industrial innovation. Finally, from the eloquent letters textile workers wrote to Franklin D. Roosevelt and the National Recovery Administration, we gained a view of the New Deal from below. Together, retrospective and contemporary evidence revealed the social logic that underlay daily practices and suggested an analysis that distinguished one epoch from another in a broad process of technological, managerial, and cultural change.[3]

Farmers and the Transformation of the Georgia Upcountry, 1850–1890 (New York, 1983). For an excellent discussion of mill building, see David L. Carlton, *Mill and Town in South Carolina, 1880–1920* (Baton Rouge, La., 1982). Earlier studies include Holland Thompson, *From the Cotton Fields to the Cotton Mills: A Study of the Industrial Transition of North Carolina* (New York, 1906); and Broadus Mitchell, *The Rise of Cotton Mills in the South* (Baltimore, Md., 1921).

[2] Jeannette P. Nichols, "Does the Mill Village Foster Any Social Types?" *Social Forces*, 2 (1923): 350–57; and Paul Blanshard, *Labor in Southern Cotton Mills* (New York, 1927), 86. This image found its most lasting expression in the work of Wilbur J. Cash. See his *Mind of the New South* (New York, 1941). A more complex understanding of southern mill workers has begun to emerge from the efforts of recent scholars. See Bess Beatty, "Textile Labor in the North Carolina Piedmont: Mill Owner Images and Worker Response, 1830–1900," *Labor History*, 25 (1984): 485–503; Douglas DeNatale, "Traditional Culture and Community in a Piedmont Textile Mill Village" (M.A. thesis, University of North Carolina, Chapel Hill, 1980); Dolores Janiewski, *Sisterhood Denied: Race, Gender, and Class in a New South Community* (Philadelphia, 1985); Mary Frederickson, "A Place to Speak Our Minds: The Summer School for Women Workers" (Ph.D. dissertation, University of North Carolina, Chapel Hill, 1981); Cathy Louise McHugh, "The Family Labor System in the Southern Textile Industry, 1880–1915" (Ph.D. dissertation, Stanford University, 1981); Melton Alonza McLaurin, *Paternalism and Protest: Southern Cotton Mill Workers and Organized Labor, 1875–1905* (Westport, Conn., 1971); Dale Newman, "Work and Community Life in a Southern Textile Town," *Labor History*, 19 (1978): 204–25; Valerie Quinney, "Childhood in a Southern Mill Village," *International Journal of Oral History*, 3 (1982): 167–92; and John Selby, "Industrial Growth and Workers' Protest in a New South City: High Point, North Carolina, 1859–1959" (Ph.D. dissertation, Duke University, 1984).

[3] Researchers from the Southern Oral History Program at the University of North Carolina, Chapel Hill, have conducted interviews with 205 mill workers throughout the North and South Carolina Piedmont. The southern textile industry stretched in a great arc from Danville, Virginia, to Georgia

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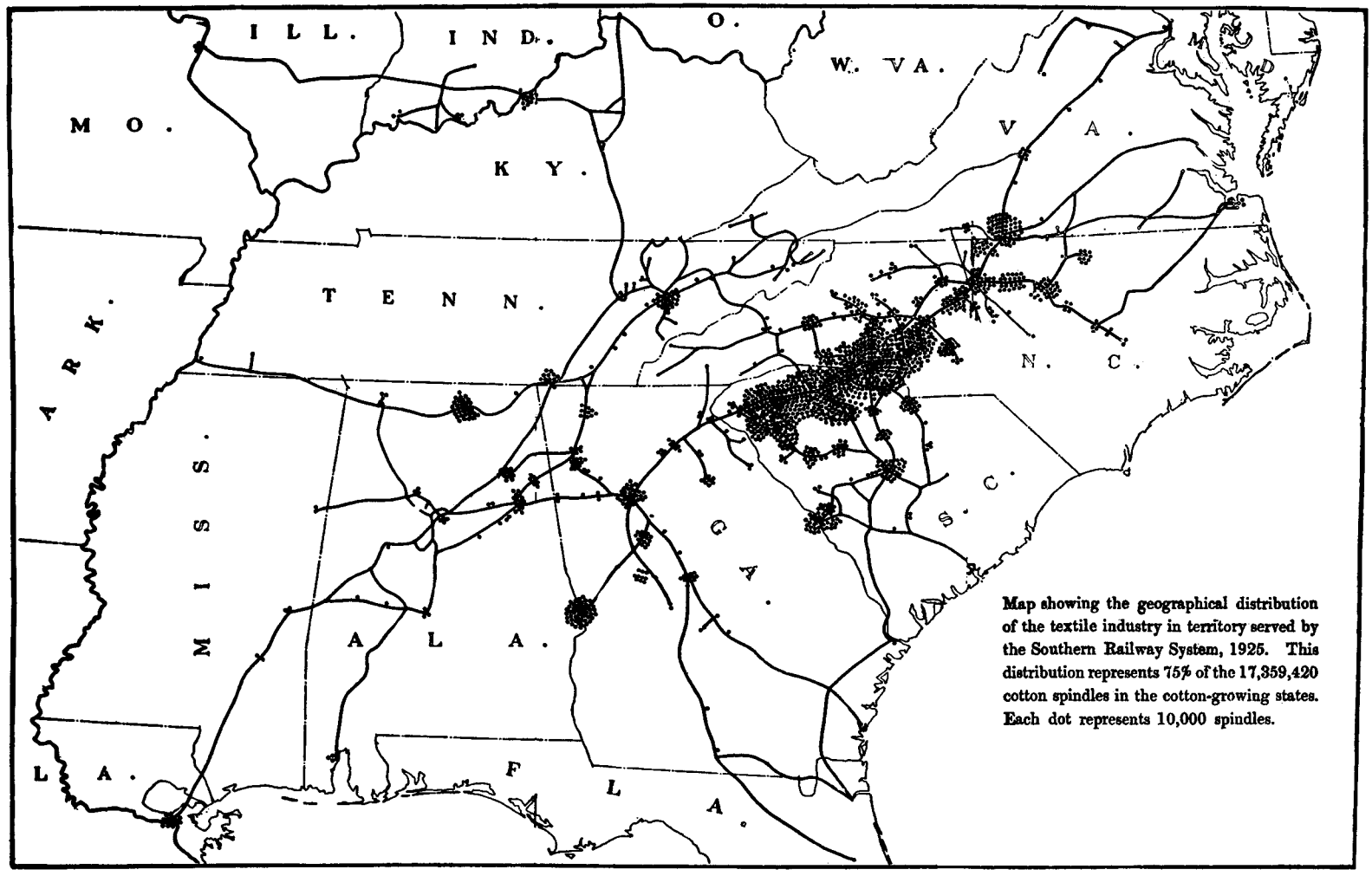
NOTHING BETTER SYMBOLIZED THE NEW INDUSTRIAL ORDER than the mill villages that dotted the Piedmont landscape. Individual families and small groups of local investors built and owned most of the early mills. Run by water wheels, factories flanked the streams that fell rapidly from the mountains toward the Coastal Plain. Of necessity, owners provided housing where none had been before. But the setting, scale, and structure of the mill village reflected rural expectations as well as practical considerations. Typically, a three-story brick mill, a company store, and a superintendent's house were clustered at one end of the village. Three- and four-room frame houses, owned by the company but built in a vernacular style familiar in the countryside, stood on lots that offered individual garden space, often supplemented by communal pastures and hog pens. A church, a company store, and a modest schoolhouse completed the scene. By 1910 steam power and electricity had freed the mills from their dependence on water power, and factories sprang up on the outskirts of towns along the route of the Southern Railway. Nevertheless, the urban mill village retained its original rural design. Company-owned villages survived in part because they fostered management control. Unincorporated "mill hills" that surrounded towns such as Charlotte and Burlington, North Carolina, and Greenville, South Carolina, enabled owners to avoid taxes and excluded workers from municipal government. But the mill village also reflected the workers' heritage and served their needs.[4]

Like the design of the mill village, the family labor system helped smooth the path from field to factory. On farms women and children had always provided

and Alabama. We chose to focus mainly on the Carolina Piedmont because this region contained the greatest concentration of textile mills. See Jules Backman and M. R. Gainsbrugh, *Economics of the Cotton Textile Industry* (New York, 1946), 169. Most of the interviews were conducted in Durham, Bynum, Burlington, Carrboro, Catawba County, and Charlotte, North Carolina; Greenville, South Carolina; and Elizabethton, Tennessee. These sites provided a cross section of the types of mill communities—from rural, company-owned mill towns to cities ringed by unincorporated mill villages. We interviewed an equal number of men and women; because blacks were excluded from most mill jobs, all but six of the mill hands we talked to were white. They fell mainly into three age groups: 26 percent were born between 1891 and 1900, 41 percent between 1901 and 1910, and 25 percent between 1911 and 1920. Twenty-four percent began work before the age of fourteen, and more than a third of that number held their first cotton mill job before they were ten years old. Roughly half of the people we talked to were the first in their families to work in the mills; the other half were the children of mill hands. Most of the children of those we interviewed abandoned the mills to seek other manufacturing and service jobs. Overall, then, our interviews represent a collective oral biography of the working men and women who transformed the Piedmont from a faltering agricultural zone into an expansive industrial region. Unless otherwise noted, the interviews are on deposit at the Louis R. Wilson Library, University of North Carolina, Chapel Hill, Southern Historical Collection. For a description of the Piedmont Industrialization Project and an acknowledgement of the researchers who participated in it, see Jacquelyn Dowd Hall, "An Oral History of Industrialization: Learning by Listening," *Institute for Research in Social Science Newsletter*, April 1981, pp. 5–9. Besides our coauthors mentioned above, those who made extensive contributions to the project include Cathy Abernathy, Della Coulter, Douglas DeNatale, Patty Dilley, Mary Frederickson, Amy Glass, Brent Glass, Rosemarie Hester, Glenn Hinson, Dolores Janiewski, Lynn Hudson, Beverly Jones, Cliff Kuhn, Lanier Rand, and Allen Tullos.

[4] Local investors often received additional capital to build mills from northern machine manufacturers and commission houses. See Mitchell, *Rise of Cotton Mills*, 241–42, 247. On mill location and village design, see Ralph R. Triplette, Jr., "One-Industry Towns: Their Location, Development, and Economic Character" (Ph.D. dissertation, University of North Carolina, Chapel Hill, 1974), 56–68; Brent Glass, "Southern Mill Hills: Design in a 'Public' Place," in Doug Swaim, ed., *Carolina Dwelling: Towards Preservation of Place, In Celebration of the North Carolina Vernacular Landscape* (Raleigh, N.C., 1978), 138–49; and Helen Bresler, "Industrial Vernacular Architecture: The Mill Villages of Glencoe and Bynum" (unpublished typescript, University of North Carolina, Chapel Hill, 1979).

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SOURCE: Marjorie A. Potwin, *Cotton Mill People of the Piedmont* (New York, 1927), frontispiece.
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essential labor, and mill owners took advantage of these traditional roles. They promoted factory work as a refuge for impoverished women and children from the countryside, hired family units rather than individuals, and required the labor of at least one worker per room as a condition for residence in a mill-owned house. But this labor system also dovetailed with family strategies. The first to arrive in the mills were those least essential to farming and most vulnerable to the hazards of commercial agriculture: widows, female heads of households, single women, and itinerant laborers. By the turn of the century, families headed by men also lost their hold on the land. Turning to the mills, they sought not a "family wage" that would enable a man to support his dependents but an arena in which parents and children could work together as they had always done.[5]

The deployment of family labor also helped maintain permeable boundaries between farm and mill. The people we interviewed moved with remarkable ease from farming to mill work and back again or split their family's time between the two. James Pharis's father raised tobacco in the Leaksville-Spray area of North Carolina until most of his six children were old enough to obtain mill jobs. The family moved to a mill village in the 1890s because the elder Pharis "felt that all we had to do when we come to town was to reach up and pull the money off of the trees." From the farm Pharis saved his most valuable possession: his team of horses. While the children worked in the mill, he raised vegetables on a plot of rented ground and used his team to do "hauling around for people." Betty Davidson's landowning parents came up with the novel solution of sharing a pair of looms. "My father would run the looms in the wintertime," Davidson remembered, "and go to and from work by horseback. And in the summertime, when he was farming, my mother run the looms, and she stayed in town because she couldn't ride the horse. Then, on the weekends, she would come home."[6]

This ability to move from farming to factory work—or combine the two—postponed a sharp break with rural life. It also gave mill workers a firm sense of alternative identity and leverage against a boss's demands. Lee Workman recalled his father's steadfast independence. In 1918 the superintendent of a nearby cotton mill came to the Workmans' farm in search of workers to help him meet the demand for cloth during World War I. The elder Workman sold his mules and cow but, contrary to the superintendent's advice, held on to his land. Each spring he returned to shoe his neighbors' horses, repair their wagons and plows, and fashion the cradles they used to harvest grain. "He'd tell the superintendent, 'You can just get somebody else, because I'm going back to make cradles for my friends.' Then he'd come back in the wintertime and work in the mill." This type of freedom did not sit well with the mill superintendent, but the elder Workman had the upper

[5] Janiewski, *Sisterhood Denied*, 55–65; McHugh, "The Family Labor System," 16–73; and U.S. Congress, *Report on the Condition of Woman and Child Wage-Earners in the United States*, volume 1: *Cotton Textile Industry*, 61st Cong., 2d sess., Senate Document No. 645 (Washington, D.C., 1910), 525 (hereafter, *Cotton Textile Industry*).

[6] James and Nannie Pharis, interview by Allen Tullos, Burlington, N.C., December 5, 1978, January 8, 30, 1979, p. 2; and Lloyd and Betty Davidson, interview by Allen Tullos, Burlington, N.C., February 2, 15, 1979, pp. 1–2.

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hand."'Well,' he told them, 'if you don't want to do that, I'll move back to the country and take the family.'"[7]

Although Lee Workman's father periodically retreated to the farm, his sons and daughters, along with thousands of others, eventually came to the mills to stay. There they confronted an authority more intrusive than anything country folk had experienced before. In Bynum, North Carolina, the mill owner supervised the Sunday School and kept tabs on residents' private lives. "If you stubbed your toe they'd fire you. They'd fire them here for not putting out the lights late at night. Old Mr. Bynum used to go around over the hill at nine o'clock and see who was up. And, if you were up, he'd knock on the door and tell you to cut the lights out and get into bed."[8] Along with surveillance came entanglement with the company store. Mill hands all too familiar with the crop lien once again found themselves in endless debt. Don Faucette's father often talked about it. "Said if you worked at the mill they'd just take your wages and put it in the company store and you didn't get nothing. For years and years they didn't get no money, just working for the house they lived in and what they got at the company store. They just kept them in the hole all the time."[9]

The mill village undeniably served management's interests, but it also nurtured a unique workers' culture. When Piedmont farmers left the land and took a cotton mill job, they did not abandon old habits and customs. Instead, they fashioned familiar ways of thinking and acting into a distinctively new way of life.[10] This adaptation occurred at no single moment in time; rather, it evolved, shaped and reshaped by successive waves of migration off the farm as well as the movement of workers from mill to mill. Village life was based on family ties. Kinship networks facilitated migration to the mill and continued to play a powerful integrative role. Children of the first generation off the land married newcomers of the second and third, linking households into broad networks of obligation, responsibility, and concern. For many couples, marriage evolved out of friendships formed while growing up in the village. One married worker recalled, "We knowed each other from childhood. Just raised up together, you might say. All lived here on the hill, you see, that's how we met." As single workers arrived, they, too, were incorporated into the community. Mary Thompson explained that the boarding houses run by

[7] Lee Workman, interview by Patty Dilley, Brookford, N.C., June 1977, p. 7.

[8] John Wesley Snipes, interview by Brent Glass, Bynum, N.C., November 20, 1976, p. 29. For other accounts of surveillance by mill owners, see H. Lanier Rand,"'I Had to Like It': A Study of a Durham Textile Community" (honors essay, University of North Carolina, Chapel Hill, 1977), 66–72; and Gary Richard Freeze, "Master Mill Man: John Milton Odell and Industrial Development in Concord, North Carolina, 1877–1907" (M.A. thesis, University of North Carolina, Chapel Hill, 1980), 59–82. For the argument that such personal involvement seldom translated into benevolent paternalism, see Beatty, "Textile Labor in the North Carolina Piedmont," 485–503.

[9] Daniel Augustus Tompkins, *Cotton Mill, Commercial Features: A Text Book for the Use of Textile Schools and Investors* (Charlotte, N.C., 1899), 35; and Don and Paul Faucette, interview by Allen Tullos, Burlington, N.C., January 7, 1979, p. 35.

[10] For further discussion of the ties between farm and factory and the creation of a hybrid mill village culture, see DeNatale, "Traditional Culture and Community." Studies of cultural continuity in the New England textile industry include Tamara K. Hareven, *Family Time and Industrial Time: The Relationship between the Family and Work in a New England Industrial Community* (Cambridge, 1983); Jonathan Prude, *The Coming of Industrial Order: Town and Factory Life in Rural Massachusetts, 1810–1860* (Cambridge, 1983); and Barbara M. Tucker, *Samuel Slater and the Origins of the American Textile Industry, 1790–1860* (Ithaca, N.Y., 1984).

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widowed women and older couples "were kind of family like. There ain't no place like home, but I guess that's the nearest place like home there is, a boarding house." Mill folk commonly used a family metaphor to describe village life. Hoyle McCorkle remembered the Highland Park mill village in Charlotte as a single household knit together by real and fictive kin: "It was kind of one big family; it was a 200–house family."[11]

Mill hands also brought subsistence strategies from the countryside, modifying them to meet mill village conditions. Just as farmers had tried to bypass the furnishing merchant, mill workers struggled to avoid "living out of a tin can." Edna Hargett's father planted a large garden every spring but could not afford a mule to help till the land. He made do by putting a harness around himself and having his children "stand behind and guide the plow." Louise Jones's family also gardened and raised "homemade meat." Her parents "had a big garden and a corn patch and a few chickens around the yard. We'd have maybe six or eight hens, and we'd let the hens set on the eggs and hatch chickens and have frying-size chickens, raise our own fryers." Self-sufficiency, however, was difficult to achieve, especially when every family member was working a ten- to twelve-hour day for combined wages that barely made ends meet. Even with their gardens, few families could sustain a varied diet through the winter months. As a result, pellagra was a scourge in the mill villages. Life was lived close to the bone.[12]

Under these conditions, necessity and habit fostered rural traditions of mutual aid. Although each family claimed a small plot of land, villagers shared what they grew and "live[d] in common." In late summer and early fall, they gathered for the familiar rituals of harvest and hog killing. Paul and Don Faucette remembered how it was done in Glencoe, North Carolina. "We'd kill our hogs this time, and a month later we'd kill yours. Well, you can give us some, and we can give you some. They'd have women get together down in the church basement. They'd have a quilting bee, and they'd go down and they'd all quilt. They'd have a good crop of cabbage, [and] they'd get together and all make kraut." Villagers helped one another, not with an expectation of immediate return but with the assurance of

[11] Ethel Faucette, interview by Allen Tullos, Glencoe, N.C., November 16, 1978, January 4, 1979, p. 35; Carl and Mary Thompson, interview by James Leloudis, Charlotte, N.C., July 19, 1979, pp. 30–31; Hoyle and Mamie McCorkle, interview by James Leloudis, Charlotte, N.C., July 11, 1979, pp. 1, 3, 5, 7; and DeNatale, "Traditional Culture and Community," 52–61.

[12] Ernest Hickum, interview by Allen Tullos, Greenville, S.C., March 27, 1980, p. 4; Edna Hargett, interview by James Leloudis, Charlotte, N.C., July 19, 1979, p. 5; and Louise Jones, interview by Mary Frederickson, Bynum, N.C., September 20, 1976, p. 42. On the prevalence of pellagra and other disabling diseases, see Joseph Goldberger *et al.*, "A Study of the Relation of Diet to Pellagra Incidence in Seven Textile Communities of South Carolina in 1916," *Public Health Reports*, March 19, 1920, pp. 648–713; and Edgar Sydenstricker *et al.*, "Disabling Sickness among the Population of Seven Cotton Mill Villages of South Carolina in Relation to Family Income," *Public Health Reports*, November 22, 1918, pp. 2038–51. A federal study of mill workers' household budgets conducted in 1907–08 revealed that "20 per cent of these people are living in the direst poverty. They are underfed, or underclothed, or they have not enough fire to keep them warm." Another 51 percent were "living in poverty of one degree or another. Some are barely above the starvation line; others have enough for food and clothing and a few of the other things considered as necessaries . . . yet they feel the pinch of poverty somewhere." U.S. Congress, *Report on the Condition of Woman and Child Wage-Earners in the United States*, volume 16: *Family Budgets of Typical Cotton-Mill Workers*, 61st Cong., 2d sess., Senate Document No. 645 (Washington, D.C., 1911), 170.

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community support in meeting their individual needs. "They'd just visit around and work voluntarily. They all done it, and nobody owed nobody nothing."[13]

Cooperation provided a buffer against misery and want at a time when state welfare services were limited and industrialists often refused to assume responsibility for job-related sickness and injury. It bound people together and reduced their dependence on the mill owners' charity. When someone fell ill, neighbors were quick to give the stricken family a "pounding." "They'd all get together and help. They'd cook food and carry it to them—all kinds of food—fruits, vegetables, canned goods." Villagers also aided sick neighbors by taking up a "love offering" in the mill. Edna Hargett organized such collections in the weave room at the Chadwick-Hoskins Mill in Charlotte. "When the neighbors got paid they'd come and pay us, and we'd take their money and give it to [the family of the weaver who was ill], and they'd be so proud of it, because they didn't have any wage coming in." To the people we interviewed, the village was "just one big community and one big family" whose members "all kind of hung together and survived."[14]

Community solidarity did not come without a price. Neighborliness could shade into policing; it could repress as well as sustain. Divorced women and children born out of wedlock might be ostracized, and kinship ties could give mill supervisors an intelligence network that reached into every corner of the village. Alice Evitt of Charlotte remarked that "people then couldn't do like they do now. They was talked about. My daddy would never allow us to be with people that was talked about. This was the nicest mill hill I ever lived on. If anybody done anything wrong and you reported them, they had to move." A Bynum proverb summed up the double-edged quality of village life. "If you went along, they'd tend to their business and yours, too, if you let them, your neighbors would. Tend to your business and theirs, too. And the old saying here, you know, 'Bynum's red mud. If you stick to Bynum, it'll stick to you when it rains.'"[15]

[13] Myra Page, *Southern Cotton Mills and Labor* (New York, 1929), 24; Paul and Don Faucette, interview by Allen Tullos, 29; and Charles Foster, interview by Brent Glass, Swepsonville, N.C., March 4, 1976, p. 14.

[14] Carl and Mary Thompson, interview by James Leloudis, 48–49; Edna Hargett, interview by James Leloudis, 24–25; and Hoyle and Mamie McCorkle, interview by James Leloudis, 3. Also see Quinney, "Childhood in a Southern Mill Village," 174, 178, 184, 187–88. Such rituals of sharing were known as "poundings" because by custom each family contributed a pound of food or other goods. Most studies of the textile industry overlook these forms of interdependence and emphasize the importance of paternalism and dependency. Particularly significant in this regard are Mitchell, *Rise of Cotton Mills*; Frank Tannenbaum, *Darker Phases of the South* (New York, 1924); and Harry Boyte, "The Textile Industry: Keel of Southern Industrialization," *Radical America*, 6 (1972): 4–49. An equally influential, if contradictory, theme is that of mill workers' "individualism." See, especially, Cash, *The Mind of the South*, 32–46, 249–50, 365, 397.

[15] Alice Evitt, interview by James Leloudis, Charlotte, N.C., July 18, 1979, pp. 8, 11; and John Wesley Snipes, interview by Douglas DeNatale, Bynum, N.C., August 22, 1979, untranscribed, tape index, p. 3. On the treatment of divorced women, illegitimate children, and unwed mothers, see Edna Hargett, interview by James Leloudis, 20–21, 58; and Carl and Mary Thompson, interview by James Leloudis, 22–23. Vinnie Partin knew a young woman in Burlington who suffered the full force of the community's wrath. "There was a girl that worked in the spinning room there, and she got pregnant and wasn't married. So everybody there just banded together and was going to quit if they didn't fire her, and they had to fire her." Vinnie Partin, interview by Valerie Quinney and Joan Sherman, Carrboro, N.C., July 3, 1975, pp. 28–29. For a discussion of the use of family networks for purposes of discipline and control, see DeNatale, "Traditional Culture and Community," 62–70; and Newman, "Work and Community Life," 212. For a comparative perspective, see Ellen Ross, "Survival Networks: Women's

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Given such tensions, we were struck by how little ambivalence surfaced in descriptions of mill village life. Recollections of factory work were something else again, but the village—red mud and all—was remembered with affection. The reasons are not hard to find. A commitment to family and friends represented a realistic appraisal of working people's prospects in the late nineteenth- and early twentieth-century South. Only after World War II, with the expansion of service industries, did the Piedmont offer alternatives to low-wage factory work to more than a lucky few. Until then, casting one's lot with others offered more promise and certainly more security than the slim hope of individual gain. To be sure, mill people understood the power of money; they struggled against dependency and claimed an economic competence as their due. Nevertheless, they had "their own ideas . . . about what constitute[d] the 'good life.'" Communal values, embodied in everyday behavior, distanced mill folk from the acquisitiveness that characterized middle-class life in New South towns.[16]

This is not to say that mill village culture destroyed individuality. On the contrary, it conferred status and dignity that the workplace could seldom afford. Although mill ways encouraged group welfare at the expense of personal ambition, they did support individual accomplishment of a different sort. The practice of medicine provides one example, music another.

Folk medicine formed an important part of workers' "live-at-home" culture. Until well into the twentieth century, mill hands simply could not afford medical fees; besides, they viewed doctors with distrust and fear. In emergencies, the village turned to its own specialists. Among the earliest of these in Bynum was Louise Jones's mother, Madlena Riggsbee. "She was what you'd say was a midwife. She could just hold up under anything. Unless they were bound and compelled to have the doctor, they'd usually get her to go." In the 1920s and 1930s, the company retained the services of a physician, paid for with funds withheld from workers' checks. But in the eyes of the villagers, he was a partner—indeed a junior partner—to Ida Jane Smith, a healer and midwife who was one of the most respected figures in the community. "Lord, she was a good woman," Carrie Gerringer recalled. "She knowed more about younguns than any doctor."[17]

If the midwife was the most prestigious member of the female community, the musician held that place among men. String bands had been a mainstay of country gatherings, and they multiplied in the mill villages where musicians lived closer together and had more occasions to play. Mastery of an instrument brought a man fame as the local "banjo king" or expert guitar picker. Musicians sometimes played simply for their own enjoyment. Paul Faucette and a small group of friends and kinfolk used "to get together on the porch on Saturday night and just have a big

Neighbourhood Sharing in London before World War I," *History Workshop Journal*, 15 (1983): 14–18; and Laurel Thatcher Ulrich, *Good Wives: Image and Reality in the Lives of Women in Northern New England, 1650–1750* (New York, 1983), 51–67.

[16] Marjorie A. Potwin, *Cotton Mill People of the Piedmont: A Study in Social Change* (New York, 1927), 17.

[17] Louise Jones, interview by Mary Frederickson, 27, 58; Carrie Gerringer, interview by Douglas DeNatale, Bynum, N.C., August 11, 1979, p. 25; and DeNatale, "Traditional Culture and Community," 70–80.

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time." On other occasions, they performed for house dances and community celebrations. Harvey Ellington remembered that on Saturday night "you'd have a dance in somebody's house—they'd take the beds and all out, and then we'd just play." The dance might end before midnight, but the musicians' performance often continued into the morning. "We'd be going home and decide we didn't want to go to bed. So we'd take the fiddle and the guitar and the banjo and stop at the corner and harmonize—do what they call serenade. The people would raise the windows and listen. That's the best sounding music, wake up at night and hear somebody playing."[18]

Special talents won Harvey Ellington and Madlena Riggsbee places of honor in their neighbors' memories. But most villagers never achieved such distinction. They lived in quiet anonymity, often guided and strengthened by religious faith. Most textile workers were evangelical Protestants, and many worshipped in churches built and financed by factory owners. On one level, these churches proved helpful, maybe even essential, to the mills. Like their counterparts in other industrializing societies, they inculcated the moral and social discipline demanded by factory life. Still, there was another side to evangelical religion, one that empowered the weak, bound them together, and brought them close to God. At springtime revivals, faith turned to ecstasy. "People got happy and they shouted. They'd sing and hug each other—men and women both." When the Holy Spirit moved individuals to confessions of sin, the entire body of worshippers joined in thanksgiving for God's saving grace.[19]

THE PHYSICAL AND SOCIAL GEOGRAPHY of the mill village, then, was less a product of owners' designs than a compromise between capitalist organization and workers' needs. For a more clear-cut embodiment of the manufacturers' will, we must look to the factory. The ornate facades of nineteenth-century textile mills reflected their builders' ambitions and the orderly world they hoped to create. The mill that still stands at Glencoe is an excellent example. Situated only a few hundred yards from the clapboard houses that make up the village, the mill is a three-story structure complete with "stair tower, corbelled cornice, quoined stucco corners, and heavily stuccoed window labels." In contrast to the vernacular form of the village, the architecture of the factory, modeled on that of New England's urban mills, was highly self-conscious, formal, and refined.[20]

At Glencoe, and in mills throughout the Piedmont, manufacturers endeavored to shape the southern yeomanry into a tractable industrial workforce. Workers'

[18] Roy Lee and Mary Ruth Auton, interview by Jacquelyn Hall, Maiden, N.C., February 28, 1980, p. 31; Paul and Don Faucette, interview by Allen Tullos, 31; and Harvey Ellington and Sam Pridgen, interview by Allen Tullos, Oxford, N.C., March 1, April 5, 1979, pp. 18–19, 35–36. Also see DeNatale, "Traditional Culture and Community," 80–87.

[19] George and Tessie Dyer, interview by Lu Ann Jones, Charlotte, N.C., March 5, 1980, p. 10. Also see Quinney, "Childhood in a Southern Mill Village," 184–86. For two perspectives on the social meaning of evangelical religion, see Paul E. Johnson, *A Shopkeeper's Millennium: Society and Revivals in Rochester, New York, 1815–1837* (New York, 1978); and Donald G. Mathews, *Religion in the Old South* (Chicago, 1977).

[20] Glass, "Southern Mill Hills," 139.

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attitudes toward factory labor, like those toward village life, owed much to the cycles and traditions of the countryside. Owners, on the other hand, sought to substitute for cooperation and task orientation a labor system controlled from the top down and paced by the regular rhythms of the machine. Barring adverse market conditions, work in the mills varied little from day to day and season to season. Workers rose early in the morning, still tired from the day before, and readied themselves for more of the same. For ten, eleven, and twelve hours they walked, stretched, leaned, and pulled at their machines. Noise, heat, and humidity engulfed them. The lint that settled on their hair and skin marked them as mill workers to the outside world. The cotton dust that silently entered their lungs could also kill them.

Owners enforced this new pattern of labor with the assistance of a small coterie of supervisors. As a rule, manufacturers delegated responsibility for organizing work and disciplining the help to a superintendent and his overseers and second hands. A second hand in a pre–World War I mill recalled, "You had the cotton, the machinery, and the people, and you were supposed to get out the production. How you did it was pretty much up to you; it was production management was interested in and not how you got it." Under these circumstances, supervision was a highly personal affair; there were as many different approaches to its problems as there were second hands and overseers. As one observer explained, "There was nothing that could be identified as a general pattern of supervisory practice."[21]

At times, discipline could be harsh, erratic, and arbitrary. This was particularly true before 1905, when most workers in southern mills were women and children. Even supervisors writing in the *Southern Textile Bulletin* admitted that "some overseers, second hands, and section men have a disposition to abuse the help. Whoop, holler, curse, and jerk the children around." James Pharis remembered that "you used to work for the supervisor because you were scared. I seen a time when I'd walk across the road to keep from meeting my supervisor. They was the hat-stomping kind. If you done anything, they'd throw their hat on the floor and stomp it and raise hell."[22]

In the absence of either state regulation or trade unions, management's power seemed limitless, but there were, in fact, social and structural constraints. Although manufacturers relinquished day-to-day authority to underlings, they were ever-present figures, touring the mill, making decisions on wages and production quotas, and checking up on the help. These visits were, in part, attempts to maintain the appearance of paternalism and inspire hard work and company loyalty. At the same time, they divided power in the mill. Workers had direct access to the owner and sometimes saw him as a buffer between themselves and supervisors, a "force that could bring an arbitrary and unreasonable [overseer] back into line." Mack Duncan recalled that in the early years "most all the mill owners seemed like they had a little milk of human kindness about them, but some

[21] Glenn Gilman, *Human Relations in the Industrial Southeast: A Study of the Textile Industry* (Chapel Hill, N.C., 1956), 142.

[22] *Southern Textile Bulletin* (hereafter, *STB*), November 2, 1911, p. 7; and James Pharis, interview by Cliff Kuhn, Burlington, N. C., July 24, 1977, p. 41.

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Workers at the Vivian Mill, Cherryville, N.C., ca. 1908. In 1880, adult women and children comprised 72 percent of the southern textile labor force. By 1905, that number had declined to 55 percent, primarily because of the entry of more adult men into the mills. See Walter B. Palmer, "Woman and Child Workers in Cotton Mills," *Quarterly Publications of the American Statistical Association*, 12 (1911): 590. Photograph reproduced courtesy of the Library of Congress, Washington, D.C.

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of the people they hired didn't. Some of the managers didn't have that. They were bad to exploit people." Under these circumstances, the commands of an overseer were always subject to review. Workers felt free to complain about unjust treatment, and owners, eager to keep up production, sometimes reversed their lieutenants' orders. Federal labor investigators reported in 1910 that "when an employee is dissatisfied about mill conditions he may obtain a hearing from the chief officer of the mill . . . and present his side of the case. Not infrequently when complaints are thus made, the overseer is overruled and the operative upheld."[23]

Authority on the shop floor was further complicated by social relations in the mill village. Before the introduction of industrial engineers and college-trained foremen in the 1920s and 1930s, most supervisors worked their way up through the ranks. Personal and occupational life were inseparable; supervisors knew the mill, the various jobs, and the ways of the workers. Edgar Moore, superintendent of the Bynum mill from 1904 to 1955, was carefully picked and groomed for his job. Relatives recalled how he attained his position. "They told Edgar, 'Now if you'll come down there and work through the mill, start at the first, just learn the machinery, when you get through with it, I'll put you overseer.' They seen something in him. And he was a brilliant fellow. The company helped him, and he worked through the mill and got to be superintendent."[24]

As time went on, Moore became known for his threats to fire workers at the least provocation, but he seldom acted on his words. He was involved both inside and outside the mill in relationships other than that of worker and manager. Like the rest of the villagers, Moore relied on the healing skill of Ida Jane Smith. He also played in the village string band, where he took a back seat to a drifter who was Bynum's best musician. Most important, Moore's brothers and sister, nieces and nephews, had married established Bynum residents and newcomers from other mills. Those kinship ties gave him access to details of workers' private lives, but they also meant that blatant injustice on his part might have repercussions on his own family.[25] In their daily lives, managers whose interests were tied to those of owners had to meet the pressures of a community to which the owners did not belong. As a result, overseers were sometimes as loyal to the workers as to the company. Eva Hopkins's father, an overseer in the Mercury mill in Charlotte, "worked with the help. He didn't work against them. He didn't work just for the company, he worked with the people." When a mill owner issued an unpopular order, such an overseer might pass the order along but yield to workers' pressure not to enforce it. Mildred Edmonds, who worked in the Piedmont Heights plant in Burlington, remembered that "they made a rule up there one time. They said you couldn't talk to the other one on the other side of you. Well, the bossman went around, and he told all of them. He got down to me, and he said, 'Mil, I'll have to tell you but I

[23] Gilman, *Human Relations*, 146–47; Mack Duncan, interview by Allen Tullos, Greenville, S.C., August 30, 1979, p. 60; and *Cotton Textile Industry*, 608.

[24] Flossie Moore Durham, interview by Mary Frederickson and Brent Glass, Bynum, N.C., September 2, 1976, pp. 22–23; and Frank Durham, interview by Douglas DeNatale, Bynum, N.C., September 10, 17, 1979, p. 5.

[25] DeNatale, "Traditional Culture and Community," 62–70.

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know it won't do no good.' I asked him what it was, and he said, 'You're not supposed to talk to each other.' I said, 'God gave me a tongue, and I'm going to use it.' It didn't last long. He didn't want to do it. But you see, the man above him put that on him."[26]

A personal style of labor management posed but one obstacle to the imposition of strict discipline. Mill owners also faced the limitations of existing technology. The small size of most mills before World War I made it difficult to coordinate production in a way that kept all hands constantly at work. The people we interviewed remembered long breaks while they waited for their machines to finish a production cycle. Weavers and spinners had time to sit down and rest or visit neighbors in the mill, while doffers, most of whom were children, could play outdoors. Workers who had spare time also helped others so that all could share a few moments away from the job. Mamie Shue offered one example. "All the boys that doffed in the spinning room, they'd bring me my bobbins and then, when they got caught up, they come out there and helped me spool. When I'd catch up, we'd go out behind the mill, sit in the grass, and stay out there about two hours. We'd come down to North Charlotte and get watermelons. Carry them back up there behind the mill and sit down. Then we'd come back in and do that all over."[27]

Most prewar mills had central power systems in which a water wheel, steam engine, or single electric motor drove the machinery through an elaborate network of belts, shafts, and pulleys. At certain times of the year water-powered mills were prone to frequent stoppages. Ethel Faucette explained that, "when the water'd get low, maybe they'd stop off for an hour or two. That was in the summertime. Well, this gang of boys would get their instruments and get out there in front of the mill, and they would sing and pick the guitar and the banjo and play different kinds of string music. And maybe they'd stand an hour or two, and the water'd gain up, and they'd start back up."[28]

The introduction of steam power and electricity at the turn of the century reduced the irregularities of production, but mills remained susceptible to the problems caused by belt-driven machinery. Paul Cline of Greenville remembered that "back in them days they had that old machinery that run by belt drives and things. They couldn't run [too fast]. Them belts'd slip off, and they'd have to stop to put the belts back on. It'd break, and they'd have to get somebody to fix the belts." Such breakdowns could stop work on a single machine or in an entire section of the mill, sometimes sending workers home for the remainder of the day.[29]

Mill owners and workers alike had to accommodate themselves to a work environment not entirely of their own choosing. Factory labor did not allow the independence and flexibility of labor on the farm, but neither did it meet the

[26] Eva B. Hopkins, interview by Lu Ann Jones, Charlotte, N.C., March 5, 1980, p. 26; and Mattie Shoemaker and Mildred Edmonds, interview by Mary Murphy, Burlington, N.C., March 23, 1979, p. 23.

[27] George and Mamie Shue, interview by James Leloudis, Charlotte, N.C., June 20, 1979, pp. 44, 46. Also see Jennings J. Rhyne, *Some Southern Cotton Mill Workers and Their Villages* (Chapel Hill, N.C., 1930), 11; and *Cotton Textile Industry*, 269–72.

[28] Ethel Faucette, interview by Allen Tullos, 24–25.

[29] Paul Cline, interview by Allen Tullos, Greenville, S.C., November 8, 1979, p. 17.

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standards of rigor and regularity desired by owners. An informal compromise governed the shop floor. "We worked longer then in the mill than they do now," explained Naomi Trammell, "and made less, too. But we didn't work hard. I done all my playing in the mill."[30]

THIS TRADEOFF BETWEEN A RELATIVELY RELAXED work pace on the one hand and long hours and low wages on the other was tenuous at best. Despite manufacturers' efforts to create a secure world in the mill and village, there were recurrent symptoms of unrest. During the 1880s and 1890s, southern mill hands turned first to the Knights of Labor and then to the National Union of Textile Workers (NUTW) to defend their "freedom and liberty." In 1900 an intense conflict led by the NUTW flared in Alamance County, center of textile manufacturing in North Carolina, when an overseer at the Haw River Mill fired a female weaver for leaving her loom unattended. The next day, September 28, union members "threw up" their machines, defending the woman's right to "go when she pleased and where she pleased." By mid-October, workers at other mills throughout the county had joined in a sympathy strike.[31]

The mill owners, conveniently overstocked with surplus goods, posted armed guards around their factories, declared they would employ only nonunion labor, and threatened to evict union members from company-owned houses. Undeterred, the workers resolved to stand together as "free men and free women"; five thousand strong, they brought production in Alamance mills virtually to a halt. But by the end of November evictions had overwhelmed the NUTW's relief fund, and the Alamance mill hands were forced to accept a settlement on management's terms.[32]

The Haw River strike capped more than two decades of unrest. During those years, Populists and factory laborers challenged the power of planters, merchants, and industrialists. Between 1895 and 1902, southern Democrats turned to race baiting, fraud, and intimidation to destroy this interracial movement. The passage of state constitutional amendments disfranchising blacks and many poor whites, accompanied by a flurry of Jim Crow laws, restructured the political system, narrowing the terms of public discourse, discouraging lower-class political participation, and making it impossible for opposition movements to survive.[33]

[30] Naomi Trammell, interview by Allen Tullos, Greenville, S.C., March 25, 1980, p. 7.

[31] *Fifteenth Annual Report of the Bureau of Labor and Printing of the State of North Carolina for the Year 1901* (Raleigh, N.C., 1902), 415–16; Jerome Dowd, "Strikes and Lockouts in North Carolina," *Gunton's Magazine*, February 1901, p. 138; and Raleigh *News and Observer*, October 6, 1900, p. 1, October 18, 1900, pp. 1–2. On the organizing efforts of the Knights of Labor and the NUTW, see Melton Alonza McLaurin, *The Knights of Labor in the South* (Westport, Conn., 1978), and *Paternalism and Protest*.

[32] Dowd, "Strikes and Lockouts," 139; Raleigh *News and Observer*, October 5, 1900, p. 1, October 20, 1900, pp. 1–2, November 21, 1900, p. 1; *Alamance Gleaner*, November 1, 1900, p. 3; and *Fifteenth Annual Report of the Bureau of Labor and Printing*, 415–16. For further discussion of the strike, see McLaurin, *Paternalism and Protest*, 156–61; and Sydney Nathans, *The Quest for Progress: The Way We Lived in North Carolina, 1880–1920* (Chapel Hill, N.C., 1983), 36–37.

[33] On disfranchisement and the larger political context in which the Haw River strike occurred, see Paul D. Escott, *Many Excellent People: Power and Privilege in North Carolina, 1850–1900* (Chapel Hill, N.C., 1985), 252–62; Goodwyn, *Democratic Promise*; J. Morgan Kousser, *The Shaping of Southern Politics:*

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As prospects for collective protest diminished, Piedmont mill hands opted for a personal strategy as old as the industry itself—relocation. In Alamance County alone, more than three hundred workers left to find new jobs in South Carolina and Georgia. "Among them," reported the *Alamance Gleaner*, "are a great many excellent people who prefer to go elsewhere rather than surrender rights and privileges which they as citizens deem they should own and enjoy." In choosing to leave in search of better conditions, the Haw River workers set a pattern for decades to come. Until the end of World War I, quitting was textile workers' most effective alternative to public protest or acquiescence. One student of the southern textile industry declared that a mill hand's "ability to move at a moment's notice was his Magna Carta, Declaration of Independence, and Communist Manifesto."[34]

This movement from job to job could be touched off by any number of factors—curtailed production, a promise of higher wages, or a simple desire to move on—but it could also be a response to a perceived abuse of authority. Josephine Glenn of Burlington explained. "A lot of people in textile mills come and go. They're more or less on a cycle. They're not like that as a whole, but a lot of them are. They're dissatisfied, you might say, restless. They just go somewhere and work awhile, and, if everything don't go just like they think it should, why, they walk out. Sometimes they'd be mad, and sometimes they'd just get on a bender and just not come back. Maybe something personal, or maybe something about the work, or just whatever they got mad about. They'd just [say], 'I've had it,' and that was it." Workers expected to be treated with respect; when it was lacking, they left. George Dyer of Charlotte offered this advice: "Sometimes some boss don't like you, gets it in for you. It's best then just to quit. Don't work under conditions like that. I didn't want to work under a man that don't respect me."[35]

The decision to move was usually made by men, and it could be hard on women and children. Family ties could fray under the wear and tear of factory life. Although Edna Hargett also worked in the mills, she was evicted from her house every time her husband quit his job. "He was bad about getting mad and quitting. He was just hot-tempered and didn't like it when they wanted to take him off his job and put him on another job. When you work in the card room, you have to know how to run about every piece of machinery in there. He liked to be a slubber, and they wanted to put him on drawing or something else. Well, he didn't like to do that." Edna understood her husband's motives but finally left to settle down and rear their children on her own.[36]

Suffrage Restriction and the Establishment of the One-Party South, 1880–1910 (New Haven, Conn., 1975); and Michael Schwartz, *Radical Protest and Social Structure: The Southern Farmers' Alliance and Cotton Tenancy, 1880–1890* (New York, 1976), 281–87. Democratic leaders in South Carolina had less success than their North Carolina counterparts in restricting white, working-class political participation. See Carlton, *Mill and Town*, 215–72.

[34] *Alamance Gleaner*, November 22, 1900, p. 3, November 29, 1900, p. 3; and Blanshard, *Labor in Southern Cotton Mills*, 48.

[35] Josephine Glenn, interview by Cliff Kuhn, Burlington, N.C., June 27, 1977, pp. 7, 20; and George and Tessie Dyer, interview by Lu Ann Jones, 37.

[36] Edna Hargett, interview by James Leloudis, 57.

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Divorce, however, was uncommon. Most families stayed together, and their moves from mill to mill were facilitated by kinship and cushioned by community. A study completed in the late 1920s revealed that 41 percent of mill families had moved less than three times in ten years. Most settled families were headed by middle-aged men and women who had "just kept the road hot" before and immediately after marriage and had then stayed in a village they liked. This relatively stable core of residents made movement possible by providing the contacts through which other workers learned of job opportunities. Established residents also mitigated the ill effects of transiency and preserved ways of life that made it easy for newcomers to feel at home. Women played central roles in this process, keeping up with the events in the village, coordinating informal acts of relief, and keeping the web of social relations intact.[37]

In these ways, the Piedmont became what journalist Arthur W. Page described in 1907 as "one long mill village." Individual communities were woven together—through kinship, shared occupational experiences, and popular culture—into an elaborate regional fabric. According to Lacy Wright, who worked at Greensboro's White Oak Mill, "We had a pretty fair picture, generally speaking, of what you might say was a 200–mile radius of Greensboro. News traveled by word of mouth faster than any other way in those days, because that's the only way we had. In other words, if something would happen at White Oak this week, you could go over to Danville, Virginia, by the weekend and they'd done heard about it. It looked like it always worked out that there would be somebody or another that would carry that information all around." Rooted in a regional mill village culture, workers like Wright took the entire Piedmont as their frame of reference.[38]

Owners and workers understood that the ability to move in search of more satisfying employment could serve as a check on mill management. After a tour of North and South Carolina mills in 1906, a superintendent vented his frustrations in the pages of the *Southern and Western Textile Excelsior*. "Unfortunately," he lamented, "the help have come to think that they and not the mill owners control the situation." Writing to the *Southern Textile Bulletin* twelve years later, another superintendent complained that "such a thing as discipline in the average cotton mill is an unknown quantity." Blaine Wofford, a Charlotte mill hand, explained. "The drawback on the mill was that a whole family would get mad and quit; if one would get mad, all would quit. That kind of put the mill behind the eight ball. There'd be all those jobs and nobody on them. I guess back then the overseers had to walk lightly with them."[39]

[37] Rhyne, *Some Southern Cotton Mill Workers*, 107; Carrie Gerringer, interview by Douglas DeNatale, 19. For more on workers' mobility, see *Cotton Textile Industry*, 127. On women's community roles, see Edna Hargett, interview by James Leloudis, 23–24, 28; Ethel Faucette, interview by Allen Tullos, 28; James and Nannie Pharis, interview by Allen Tullos, 43; and Frances Latta, interview by Valerie Quinney, Carrboro, N.C., July 8, 1975, p. 11. Ellen Ross discussed similar activities among working-class women in pre–World War I London; "Survival Networks," 10–14.

[38] Arthur W. Page, "The Cotton Mills and the People," *Southern and Western Textile Excelsior* (hereafter, *SWTE*), June 8, 1907, p. 3; and Lacy Wright, interview by Bill Finger and Chip Hughes, Greensboro, N.C., March 10, 1975, pp. 21–22.

[39] "The Help Question from the Superintendent's Point of View," *SWTE*, August 4, 1906, p. 13; "From a Mill Superintendent," *STB*, May 30, 1918, p. 12; and Blaine Wofford, interview by Allen Tullos,

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Through the end of World War I, mill workers' "roving disposition" not only posed serious problems for shop floor discipline but also aggravated a chronic labor shortage. As early as 1906, manufacturers worried openly that labor turnover was leaving machines idle and shrinking profits. They had unwittingly contributed to these problems by taking part in a building boom at the turn of the century that dramatically increased the demand for labor. As jobs became more plentiful, mill hands were free to move more frequently. Workers remembered that "it wasn't no job at all to get a job then." By 1907 the annual labor turnover rate in the southern textile industry had reached 176 percent, and various mill owners estimated that anywhere from 20 to 40 percent of their employees belonged to the "floating" population. Mill worker Paul Cline summed it up in one vivid image. "My daddy had an old rooster—when he seen the wagon coming, he'd just lay down and cross his legs; he knowed it was time to move."[40]

Owners first responded to the problems of labor scarcity and control by competing more fiercely with one another. In hopes of attracting and keeping reliable workers, many mill owners raised wages. Between 1902 and 1907 the earnings of male weavers in South Carolina rose by 58 percent, those of female weavers by 65 percent, and those of spinners by 138 percent, while the cost of living increased by only 9 percent. But, if owners sought stability, they did not achieve it with wage competition. As one superintendent complained, "The more the help can earn, the less they work. . . . The prices paid at present have gotten to be so high that an average family, or individual, can make in four days what it once took a full week to earn, and the result is in most instances, such help work only four-sixths of their time. The average mill hand is inclined to care only for enough to live on and when they have made that, they rest the balance of the week."[41]

A more drastic response to the competition for workers was labor pirating. Mill owners throughout the Piedmont hired recruiters to circulate through other mill villages and induce workers to leave their jobs. "In some instances," reported the *Southern Textile Bulletin*, "men are sent to other towns to mix with the people on Saturday night, display rolls of money, and tell remarkable stories of how much they earn at this or that mill." Owners paid moving expenses and offered free housing and groceries to lure workers into their mills. But those practices, like wage increases, only exacerbated the problems they were intended to resolve. The *Southern and Western Textile Excelsior* warned mill owners that "the scarcity of help

Charlotte, N.C., February 1, 1980, untranscribed, tape index, p. 2. Also see Thomas F. Parker, "The South Carolina Cotton Mill—A Manufacturer's View," *South Atlantic Quarterly*, 8 (1909): 332.

[40] "The Mill Help Question Discussed by Mill Men," *SWTE*, April 20, 1907, p. 17; Martin Lowe, interview by Allen Tullos, Greenville, S.C., October 19, 21, 1979, p. 10; Daniel T. Rodgers, "Tradition, Modernity, and the American Industrial Worker: Reflections and Critique," *Journal of Interdisciplinary History*, 7 (1977): 663, 671; and Paul Cline, interview by Allen Tullos, 15. For more on the turn-of-the-century building boom and labor turnover, see Thompson, *From the Cotton Field to the Cotton Mill*, 71; *Cotton Textile Industry*, 126–27; "Wage Earners in Cotton Textile Industry," *STB*, September 7, 1911, pp. 4, 17; Gilman, *Human Relations*, 130; McHugh, "The Family Labor System," 19; and Carlton, *Mill and Town*, 152–53.

[41] Carlton, *Mill and Town*, 142; and "The Mill Help Question Discussed by Mill Men," 17.

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among the cotton mills is aggravated by the pernicious practice of furnishing transportation and enticing help from one mill to another. . . . You furnish transportation from another mill to yours, and another man furnishes transportation from your mill to his, and so the game goes on and maybe there are enough operatives on the trains all the time going from place to place to operate all the idle spindles in the South."[42]

Manufacturers took such warnings to heart, realizing that the solution to their problems lay not in fighting among themselves but in rethinking their relations to one another and the nature of mill management. Joining forces in 1908, they turned to their own advantage the efforts of overseers and second hands to organize a supervisors' union. Although uninvited, David Clark and representatives from the state's leading mills attended an overseers' meeting in Spray, North Carolina, and convinced those present that their interests would best be served by allying themselves with mill owners rather than seeking a separate voice. In October of that year, Clark presided over the formation of the Southern Textile Association, which offered membership to all superintendents, overseers, master mechanics, electricians, engineers, editors of textile publications, and instructors at textile schools. The association subsequently played a major role in the industry's development, serving as a conduit for the introduction of new theories of personnel management and industrial relations.[43]

The manufacturers' most ambitious undertaking in the prewar years was the implementation of company-sponsored "welfare work." Taking their cue from the National Civic Federation and major northern corporations, southern mill owners embarked on a campaign to beautify their villages and provide new social services for their employees.[44] Urged on by industry publicists such as Clark, they began substituting professional intermediaries—social workers, nurses, and teachers—for the direct surveillance of workers' lives. Welfare work was expensive and therefore gained only a limited following among small-scale manufacturers. But the Piedmont's largest mills embraced the new approach to labor-management relations with enthusiasm.

Writing in the *Southern Textile Bulletin*, manufacturers made clear the relationship between welfare work and labor mobility. One mill president explained that he and others undertook welfare work "to secure an attachment for the village to

[42] "Scamping of Labor," *STB*, June 6, 1918, p. 10; and "The Labor Problem," *SWTE*, May 26, 1906, p. 10. For more on labor pirating, see Gilman, *Human Relations*, 130; and *Cotton Textile Industry*, 126–27.

[43] Marshall Dilling, "Review of the History of the Southern Textile Association," *Textile Bulletin*, 77 (1951): 140–43; "Southern Textile Association," *American Textile Manufacturer*, October 29, 1908, p. 5; and "History of the Southern Textile Association," *STB*, November 21, 1918, p. 3.

[44] Southern textile manufacturers defined welfare work as services not directly related to production. Village beautification schemes were included but not the provision of low-rent housing, since the village itself was necessary to secure a labor force and carry on the business of the mill. For general accounts of welfare work and American business, see Stuart D. Brandes, *American Welfare Capitalism, 1880–1940* (Chicago, 1976); David Brody, "The Rise and Decline of Welfare Capitalism," in David Brody, *Workers in Industrial America: Essays on the Twentieth-Century Struggle* (New York, 1980), 48–81; and Daniel Nelson, *Managers and Workers: Origins of the New Factory System in the United States, 1880–1920* (Madison, Wis., 1975), 101–21.

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decrease the migratory tendency." W. A. Giles of Graniteville, South Carolina, advised fellow mill presidents that welfare work made employees "more loyal, better workers, better contented with their lot, and it will be found that those mills which pay more attention to work of this kind will have [the] best class of labor and more of it in times of stress." Although mill owners were no doubt sincere in their efforts to improve workers' living conditions, welfare work was "also [a] cold blooded business proposition."[45]

But how could such mundane activities as garden clubs and organized recreation accomplish mill owners' goals? Above all, welfare programs tried to redefine the neighborhood as a physical extension of the mill rather than a network of human relations. Manufacturers awarded prizes for the best flower gardens in their villages and set aside specific plots of land for the vegetable gardens workers had always tilled. They also took over many of the activities that bound workers together. They established "domestic science" classes in which girls learned the arts of housekeeping and cooking not from their mothers and grandmothers but from a professional social worker, and company nurseries replaced informal, family-based child care. Mills financed workers' "pick-up" baseball teams and organized factory leagues in an effort to transform community games into a sport sponsored by and identified with the company. They formed brass bands to take the place of village string bands and built YMCAs to structure workers' leisure. On occasion, owners outlawed the collection of "love offerings" and, in the 1920s, began to offer minimal insurance programs instead. Promotional literature for these programs promised to "eliminate passing the hat in the mill" and enable the employer "to assist the community in meeting some of its welfare problems."[46]

Welfare programs also aimed at stimulating desires for consumer goods that would compel mill hands to work regularly. Here domestic science classes did double duty. As we have already seen, workers' nonacquisitiveness could decrease productivity by promoting absenteeism and "loafing." To combat the problem, professional social workers taught mill women to make fashionable clothes, cook elaborate meals, and keep neat homes furnished with upholstered sofas and chairs. The *Southern and Western Textile Excelsior* voiced owners' confidence that, if these measures did not change the present generation of mill workers, they would at least influence their children. "Their needs must be increased to equal the rise in wages to get them to work steadily," one mill president candidly explained. "The people are not sufficiently ambitious to care to work all the time, but as we are throwing about them elevating influences their needs are growing greater and the next generation will be all right."[47]

[45] A mill president, as quoted by Gertrude Beeks, "In Southern Cotton Mills," *SWTE*, August 11, 1906, p. 18; W. A. Giles, "Welfare Work," *Textile Manufacturer*, July 7, 1910, p. 23; and E. S. Draper, "Community Work in Southern Mill Villages," *STB*, May 8, 1919, p. 31.

[46] Harriet L. Herring, *Welfare Work in Mill Villages: The Story of Extra-Mill Activities in North Carolina* (Chapel Hill, N.C., 1929), 106–218, 248–92; Potwin, *Cotton Mill People*, 151–52; and advertisement, *STB*, April 9, 1936, p. 3. The *Southern Textile Bulletin* promoted welfare work through special "Health and Happiness" issues: December 20, 1917, December 25, 1919, and November 22, 1923.

[47] Beeks, "In Southern Cotton Mills," 18.

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Despite promises that welfare work would bring "the employer and the employee into closer touch and . . . eliminate industrial unrest," results were disappointing. Many workers recognized the purpose of welfare activities and refused to participate. One mill hand, writing to the Charlotte *Labor Herald*, observed that owners had substituted "cunning" for authoritarianism in their dealings with employees. "How this could best be done, was a question of much speculation, and has caused no little experimenting," he explained. "Said some, 'We will build . . . kindergartens for the tots, and Y.M.C.A.'s for the adults. . . . Thus from the cradle to the grave, we will mould their minds and formulate their convictions. . . .' To accomplish this, they learned that a good staff of social workers was as necessary as the tusks in the mouth of a vicious wild beast."[48]

Other mill hands preserved their independence in less self-conscious ways. Mamie Shue remembered that Charlotte's Highland Park Mill "had a house fixed up for a cooking school. And we'd go down there, and they'd teach us how to cook—teach us how to make fancy meals. We just went for the fun of it. We didn't care nothing about cooking." On the whole, our interviews suggest that welfare work failed to attract workers' interest and redirect their loyalties. The carefully balanced investigations of sociologist Harriet Herring point in the same direction. North Carolina mill owners found that "the response of the villagers was so discouraging as to make the work seem not worth while." As an economic depression settled over the textile industry in the years after World War I, manufacturers became convinced that the results of welfare work did not justify the expense. Only a handful of mills initiated new programs after 1920, and many abandoned their welfare activities altogether.[49]

WORLD WAR I MARKED A TURNING POINT in the development of the southern textile industry. Stimulated by wartime demand, new mills sprang up, old ones operated around the clock, wages rose, and profits soared. But, when peace came, overexpanded businesses went into a tailspin. The situation worsened when tariff policies and the advent of textile manufacturing in other parts of the world cut into the southern industry's lucrative foreign markets. A sudden change in clothing styles added to manufacturers' troubles. Young women in the 1920s hiked their skirts six inches above the ankle, then all the way to the knee, causing consternation among their elders and panic in the textile industry. All in all, the depression that hit the rest of the country in 1929 began for textile manufacturers in the immediate postwar years.[50]

Mill officials greeted the armistice with a rollback of workers' wages. But, to the owners' surprise, mill hands refused to abandon small but cherished advances in

[48] David Clark, "Effect of Welfare Work on Industrial Unrest," *STB*, July 22, 1915, p. 5; and *Labor Herald*, August 17, 1923, p. 4.

[49] George and Mamie Shue, interview by James Leloudis, 30; and Herring, *Welfare Work in Mill Villages*, 123. For owners' doubts about the success of welfare work, see W. R. Lynch, "Evolution of Welfare Work in Southern Mill Communities," *STB*, July 8, 1915, p. 3.

[50] Liston Pope, *Millhands and Preachers: A Study of Gastonia* (New Haven, Conn., 1942) 217–18; Gilman, *Human Relations*, 177; and Gavin Wright, "Cheap Labor and Southern Textiles, 1880–1930," *Quarterly Journal of Economics*, 96 (1981): 606.

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their standard of living. When wage cuts were announced in 1919, thousands of workers joined the American Federation of Labor's United Textile Workers (UTW). "They are in deadly earnest," reported the Raleigh *News and Observer*, "and almost religiously serious in their belief in the union." Manufacturers were equally determined not to employ union members, and in many cases they simply shut their factory gates to all workers. As the conflict dragged on, threats of violence mounted. Armed strikers patrolled the mill villages, intent on enforcing community and union solidarity. Manufacturers eventually agreed to a settlement but insisted that "the adjustment . . . shall not be construed as a recognition by the mills of collective bargaining." Similar confrontations occurred throughout the Piedmont until 1921, when a severe business downturn crippled union locals and gave management the upper hand. But workers had made their message clear: mill owners would no longer be able to shore up profits simply by cutting wages.[51]

The impasse created in 1921 by hard times and workers' protests set the stage for a new era of corporate consolidation. As smaller firms went bankrupt, more aggressive competitors gobbled them up. J. Spencer Love, who took over faltering mills in Alamance County and eventually built Burlington Mills into the world's largest textile enterprise, set the pace. Love's generation led the region to ascendancy in the production of synthetics and helped effect a permanent shift of cotton manufacturing from New England to the Piedmont. These "progressive mill men" also set out to find new solutions to problems of profitability and labor control.[52] The methods they adopted aimed at altering the structure of work and breaking the bonds between supervisors and the mill village community. But their freedom of action depended on a ballooning labor supply.

As the demand for cotton fell and an agricultural depression settled over the countryside, farmers again came to the mills—in "droves," recalled John Wesley Snipes, "all of them hunting jobs." A story that made the rounds in mill villages summed up the situation. In Snipes's version, the supervisor of the Bynum mill told a desperate applicant, "'No, we ain't got no job for you, not unless somebody dies.'" As the man walked away, "this fellow fell out of the window and got killed." So he ran back and said, "'How about that man; can I have his job?'" "'No,'" replied the superintendent, "'the man that pushed him gets his [job].' They told it as a joke," Snipes concluded, "but it was rough, I'm telling you." For the first time since

[51] On the Charlotte strike, see *Charlotte Observer*, February 26, 1919, p. 4. May 28, 1919, p. 2, May 31, 1919, p. 5. For a discussion of other strikes through 1921, see George Sinclair Mitchell, *Textile Unionism and the South* (Chapel Hill, N.C., 1931), 42–53; and Herbert J. Lahne, *The Cotton Mill Worker* (New York, 1944), 205–14. Gavin Wright examined the stability of real wages; "Cheap Labor and Southern Textiles," 623.

[52] Bill Finger, "Textile Men: Looms, Loans, and Lockouts," *Southern Exposure*, 3 (1976): 54–65; and Mary Murphy, "Burlington, North Carolina," Southern Oral History Program Working Paper, University of North Carolina, Chapel Hill, 1980. Burlington Mills Corporation changed its name to Burlington Industries in 1955; University of North Carolina, Chapel Hill, Louis R. Wilson Library, North Carolina Collection, Burlington Mills Corporation, *30th Annual Report* (n.p., n.d.), 5. Between 1923 and 1933, 40 percent of the mills in New England closed down, and almost one hundred thousand of the one hundred ninety thousand workers once employed there lost their jobs. The South's share of the total number of textile workers rose from 46 to 68 percent. See George B. Tindall, *Emergence of the New South, 1913–1945* (Baton Rouge, La., 1967), 78. "Progressive mill men" was the phrase David Clark frequently used to describe industry leaders in technological and managerial innovation.

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1900, owners could press forward with innovation, unhindered by the fear that disgruntled workers could pick up and move to another mill.[53]

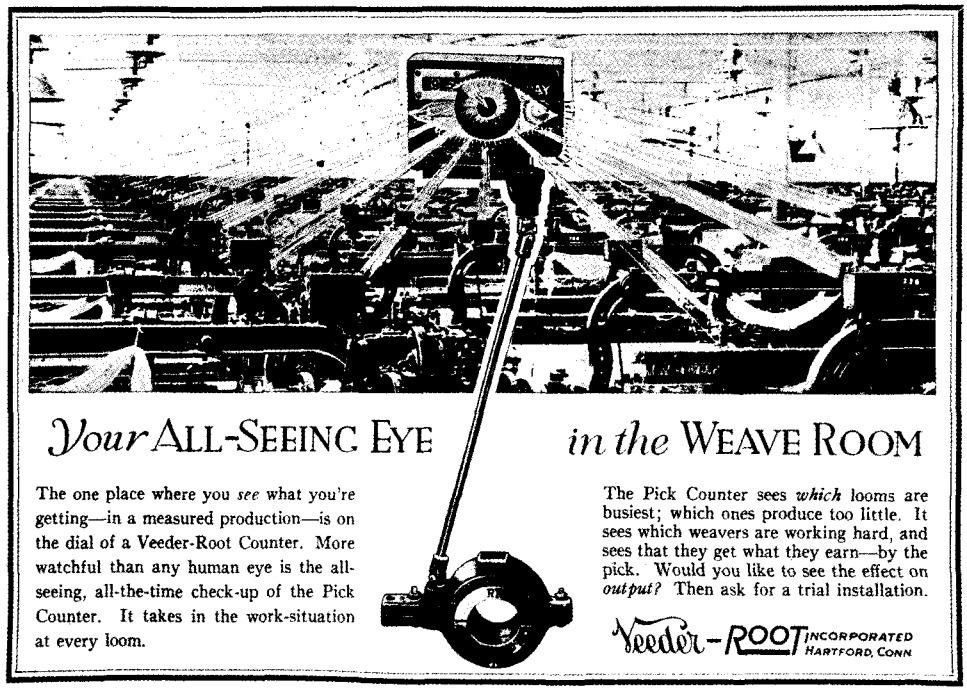
The industry looked first to simple cost-cutting measures. One approach was to run the mills around the clock. The faster old machinery could be used up and paid for, the sooner new, efficient designs could be introduced. A second strategy involved tightening operations throughout the mill. Writing in the *Southern Textile Bulletin*, a "well known superintendent" explained that a major problem was the "waste of time. . . . There are many ways of wasting time . . . too much talking, too much going after drinking water, and too much loitering around sink rooms. . . . Time is money and the waste of time is the waste of cash. . . . Margins of profit are now so close that waste of all kinds must be most carefully guarded against. Leaks cost money which . . . must, of necessity, come out of profits which would otherwise have been paid out to the owners." But "preventing leaks in mill operation" was, at best, a stopgap measure.[54] Major increases in productivity required more fundamental changes. Faster-running, labor-saving machinery had to be installed, jobs had to be reorganized, and new supervisory practices had to be put in place.

In 1924 the *Southern Textile Bulletin* announced a "Better Equipment Campaign" designed to "show the great advantage of modern machinery and methods over old style or primitive forms of machinery." Through the early 1930s, equipment manufacturers flooded the *Bulletin* and other trade journals with advertisements praising the cost effectiveness and labor-saving qualities of their machines. The Barbara-Colman Company lauded the ability of its new line of spoolers and warpers to remove "the human equation" from production by enabling mills to run with far fewer workers. Other advertisements boasted that Saco-Lowell equipment could reduce the number of men needed to run five pickers from thirteen to four: "Lost—Nine Men But They'll Never Be Missed!" When Blaine Wofford went to work in the Highland Park Mill, twenty-six men ran the speeders and slubbers in the card room. In the late 1920s the company installed machines that enabled "five

[53] John Wesley Snipes, interview by Brent Glass, 30. On the ballooning labor supply and postwar increases in unemployment, part-time work, and layoffs, see Wright, "Cheap Labor and Southern Textiles," 626; Gilman, *Human Relations*, 177, 207–11; and Lahne, *The Cotton Mill Worker*, 149–52. Although moving became less effective as a form of protest, labor mobility did not decline all at once. Sociologist Harriet Herring observed that "labor turnover did not stop or even materially diminish when the depression set in at the middle of 1920. . . . Running from wage cuts and short time was as common as running to increases had been." See Herring, *Welfare Work in Mill Villages*, 122. Turnover remained high in southern mills in 1923; the average annual rate was 190 percent. As the economic depression took hold, however, that figure declined, as it did for American industry in general. See U.S. Department of Labor, Women's Bureau, *Lost Time and Labor Turnover in Cotton Mills: Study of Cause and Extent*, Bulletin 52 (Washington, D.C., 1926), 109; Herring, *Welfare Work in Mill Villages*, 122; Rodgers, "Tradition, Modernity, and the American Industrial Worker," 674–75; and Ewan Clague, "Long-Term Trends in Quit Rates," in U.S. Bureau of Labor Statistics, *Employment and Earnings*, December 1956, pp. iii–ix.

[54] "Preventing Leaks in Mill Operations," *STB*, July 28, 1927, pp. 7, 34. On the expansion of night work and multiple shifts during the 1920s, see Martha Shields and Gavin Wright, "Night Work as a Labor Market Phenomenon: Southern Textiles in the Interwar Period," *Explorations in Economic History*, 20 (1983): 331–50.

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Advertisement for Veeder-Root pick counters, *Southern Textile Bulletin*, July 11, 1929, p. 8.

men to take off more production than the 26 was taking off with the other machinery." The new equipment kept them "balling the jack all the time."[55]

General Electric and Westinghouse encouraged manufacturers to adopt "the motor way to greater production." Mill owners attached individual electric motors to their machines and abandoned older, less reliable belt-drive systems, freeing mill operation from "the whims of a river." With motor drives, the failure of a single machine no longer stopped work in other parts of the mill. Equipment also ran at more uniform speeds, reducing the number of mechanical breakdowns and damage to the goods. When combined with efficient new machinery, the turn to electricity enabled mills to "maintain maximum production speed." "That ruined our playhouse when they got power," recalled Eula Durham of Bynum. "That tore up our playhouse."[56]

[55] H. D. Martin, "Advantages of New Equipment," *STB*, May 1, 1924, p. 7; advertisements, *STB*, February 19, 1925, p. 38, March 14, 1929, p. 47; and Blaine Wofford, interview by Allen Tullos, untranscribed, tape index, p. 2. For a discussion of the labor-saving effects of new machinery, see Boris Stern, "Mechanical Changes in the Cotton-Textile Industry, 1910 to 1936," *Monthly Labor Review*, August 1937, pp. 316–41. Increased productivity was hardly a new concern for southern textile manufacturers. From the outset, they ranked among the most enthusiastic purchasers of labor-saving equipment. Between 1894 and 1919, southern mills adopted the automatic Draper loom at nearly twice the rate of New England firms. Before the 1920s, however, efforts to improve efficiency remained piecemeal. See Irwin Feller, "The Diffusion and Location of Technological Change in the American Cotton-Textile Industry, 1890–1970," *Technology and Culture*, 15 (1974): 569–75.

[56] Advertisements, *STB*, November 27, 1924, pp. 3–8, April 28, 1921, p. 13; Eula and Vernon Durham, interview by James Leloudis, Bynum, N.C., November 29, 1978, p. 61. For discussions of the changes brought by the use of individual electric motors, see "Electric Power in the Textile Industry," *STB*, August 12, 1915, pp. 4–5; Sidney B. Paine, "Electricity and the Textile Industry," *STB*, April 26, 1928, pp. 12, 32; and H. W. Redding, "Twenty Years' Development in Electrical Equipment," *STB*, March 5, 1931, pp. 34, 46.

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Production monitoring equipment also enforced a more rigorous pace of work. Advertisements urged owners to "use Veeder-Root 'watch-dogs of the weave room' to detect looms that steal time, operatives that waste time; loom fixers that lose time for both." Such monitors would serve as the supervisor's "all-seeing eye," putting "the screws on operating costs" by forcing every worker to "face the facts of his job." Operatives might "fool the foreman," but they could not "deceive the 'silent little superintendent' on the end of the frame."[57]

Technological innovation alone could not solve all problems. Equally important was application of labor-saving methods in the organization of work. The weave room was the main target of reform. The Northrup loom, eagerly adopted by southern mills after the turn of the century, automated many weave-room tasks. Yet in an industry that had relentlessly stripped most jobs of initiative and skill, weavers remained more independent and better paid than most textile workers. The multiple-loom system undermined that privileged position. An adaptation of Frederick W. Taylor's scientific management, the system parceled weaving into numerous tasks performed at low pay by workers with little training. Weavers, left with the simple job of mending broken threads, could tend more looms, but at the expense of control over the pace and methods of their labor. Yoked to their work stations, they no longer moved about the mill, filling their own batteries and doffing their own cloth—and finding opportunities for conversation and companionship. Most important, the multiple-loom system brought unemployment and, when piece rates were cut, a decrease in earnings.[58]

With this new division of labor in place, industrial engineers concentrated on maximizing each worker's productivity. Sam Finley, a loom fixer in Marion, North Carolina, remembered how it was done. "They got a stopwatch, and they followed them around. They figured out exactly how long it took you to tie that thread and start the loom up. They figured right down to the tick of that stopwatch. Then they expected you to stretch it out a little bit, to do a little more. You couldn't please them. The more you done, the more they wanted done." To Finley, the multiple-loom system was simplicity itself: "Give them more work for no more pay."[59]

[57] For advertisements of Veeder-Root counters, see *STB*, August 2, 1928, p. 16, July 11, 1929, p. 28, June 11, 1931, p. 17, December 6, 1928, p. 18; and Oscar E. Elsas, "Piece Rate Wage System in Cotton Mills," *STB*, April 29, 1915, p. 3. Also see "Root Loom Counters," *STB*, July 24, 1924, pp. 20–22.

[58] Elliott Dunlap Smith, "Lessons of the Stretchout: A Preliminary Report of a Study of Some Human Problems in the Management of Technological Change," *Mechanical Engineering*, 56 (1934): 73–80; Elliott Dunlap Smith and Richmond Carter Nyman, *Technology and Labor: A Study of the Human Problems of Labor Saving* (New Haven, Conn., 1939); Albert Palmer, "Cost of Manufacturing and the Multiple Loom System of Operation: Weaver and His Work Present Large Opportunities for Savings," *Textile World*, February 2, 1929, pp. 183–84, 287; and J. M. Barnes, "The Labor Extension System," *STB*, July 5, 1928, pp. 7, 35. Between 1924 and July 1933, wages for male weavers fell from an average of 36 cents per hour to 24 cents; those for women dropped from 31 to 22 cents. See A. F. Hinrichs and Ruth Clem, "Historical Review of Wage Rates and Wage Differentials in the Cotton-Textile Industry," *Monthly Labor Review*, May 1935, p. 1171.

[59] Sam Finley, interview by Sam Howie, Marion, N.C., September 11, 1976, Appalachian Oral History Project, Appalachian State University, Boone, N.C., p. 10. On time-motion studies, see Smith and Nyman, *Technology and Labor*, 82–91; and Henry Joseph Rehn, *Scientific Management in the Cotton Textile Industry* (Chicago, 1934), 109–12. Rehn's work is a private edition distributed by the University of Chicago Libraries.

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Along with new technologies and the reorganization of work came an attack on the idiosyncratic relations of the shop floor. In part because of prodding from the *Southern Textile Bulletin*, larger mills began replacing men who had come up through the ranks with young, college-trained supervisors. Those who still rose from production jobs did so by taking night-school or correspondence courses in which they learned to "handle men" through "diplomacy" rather than "loud swearing" and "hat stamping." Instead of the mechanical skills and personal relations on which their predecessors had relied, the newly schooled supervisors derived authority solely from the owners and passed orders along through a strict chain of command. No longer could a loom fixer like Sam Finley aspire to the post of weave-room supervisor. In the old days, he explained, "when it came time when they needed a supervisor, you'd get that job." But, during the 1920s, such positions went to men with textile degrees who served only a brief stint on the shop floor and "didn't know a thing in the world about weaving. They'd bring them down there and in two or three weeks they'd have a supervisor out of him. What they wanted was somebody when they'd whistle they'd jump. Make the other jump. And that's the difference."[60]

As in other aspects of the industry's evolution, J. Spencer Love of Burlington Mills led the way. Harry Rogers remembered that Love's new, college-trained supervisors "were strictly for Burlington Mills. A human being didn't mean too much to them. The machinery did. They changed their attitude, I reckon. They kept close count of all the figures, and that's all they knew, was the figures. 'How much production did you get? How was quality?' They tried to put across that they were interested in the human side of it, but I don't think they were." Under this regime, overseers and second hands were less likely to ally themselves with workers by questioning company policy, and workers could no longer look to the superintendent or the owner as a mediator between themselves and their immediate supervisors.[61]

Social critics in the 1920s were quick to point out the evils of the mill village, but few noted the extraordinary capacity for surveillance embedded in impersonal machines or imagined the psychological havoc that rationalization could bring. Workers, on the other hand, remembered conditions that "just kept getting worse and worse." The "stretch-out" was their term for the cumulative changes that set them tending machines "by the acre," filled every pore of the working day, and robbed them of control over the pace and methods of work. "There's many a times I dreamt about it," Edna Hargett recalled. "Sometimes you'd be up on your job,

[60] "Management of Help," *STB*, November 2, 1911, p. 6; and Sam Finley, interviewed by Sam Howie, 6. Beginning in July 1928, the *Southern Textile Bulletin* printed numerous articles on the question of "why the experienced man at the age of 40 years is being turned down for younger men" in promotions to overseer. See, for example, "Can You Tell Him?" *STB*, July 26, 1928, p. 16. Southern mills began experimenting with new management styles as early as the 1910s, primarily as a way of reducing labor turnover. The technological changes of the 1920s, however, demanded that those experiments be applied systematically. As Elliott Smith observed, "The refinement of laboring methods called for a parallel refinement in the techniques of executive control, and that in turn required a new type of manager." See Smith, "Lessons of the Stretchout," 79–80. For a mill manager's perspective, compare W. M. McLaurine, "Modern Methods Demand Modern Men," *STB*, April 2, 1931, pp. 14, 32.

[61] Harry Rogers, interview by Cliff Kuhn, Burlington, N.C., July 21, 1977, p. 16.

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and other times you'd be behind. So I just sweated it out in my dreams like I did when I was there on the job, wanting to quit, but I knew I couldn't afford to."[62]

In 1927 resistance to management tactics by individuals and small groups gave way to labor conflict on an unprecedented scale. The battle opened in Henderson, North Carolina, where workers struck for restoration of a bonus withdrawn three years before. Then on March 12, 1929, young women in a German-owned rayon plant in Elizabethton, Tennessee, touched off a strike wave that spread quickly into the Carolinas. The involvement of the communist-led National Textile Workers Union and the shooting deaths, first of the police chief and then of Ella May Wiggins, the strikers' balladeer, brought Gastonia, North Carolina, a special notoriety. But the carnage was even worse in nearby Marion, where deputies opened fire on demonstrators, wounding twenty-five and killing six. In 1930, revolt hit the massive Dan River Mill in Virginia—a model of welfare capitalism.[63]

Responding to these workers' initiatives, the UTW tried to remedy its neglect of southern labor. Most energetic was the American Federation of Hosiery Workers, an autonomous UTW affiliate, represented by Alfred Hoffman, an intrepid organizer who popped up in virtually every trouble spot until his militancy landed him in a Marion jail. But even Hoffman usually arrived after the fact, and a number of the less well known but more successful walkouts ran their course with no official union involvement at all. In 1929, thousands of South Carolina workers formed their own relief committees, held mass meetings, and negotiated modifications in the stretch-out—all without help from the UTW. Similarly, in 1932, in High Point, North Carolina, hosiery workers sparked sympathy strikes at textile mills and furniture plants and used automobile caravans to spread walkouts to nearby towns. Fearing a "revolution on our hands," officials conceded most of the workers' demands.[64]

[62] Perry Hicks, interview by Sam Howie, Marion, N.C., December 31, 1975, Appalachian State University Oral History Program, Appalachian State University, Boone, N.C., p. 3; Robert Blauner, *Alienation and Freedom: The Factory Worker and His Industry* (Chicago, 1964), 66; and Edna Hargett, interview by James Leloudis, 64. On criticism of the mill village, see Daniel Joseph Singal, *The War Within: From Victorian to Modernist Thought in the South, 1919–1945* (Chapel Hill, N.C., 1982), 3–10, 115–52, 302–38; and Harriet L. Herring, "Cycles of Cotton Mill Criticism," *South Atlantic Quarterly*, 28 (1929): 113–25.

[63] For surveys of these strikes, see Irving Bernstein, *The Lean Years: A History of the American Worker, 1920–1933* (Boston, 1972), 1–43; and Tom Tippett, *When Southern Labor Stirs* (New York, 1931). For more recent studies, see David Painter, "The Southern Labor Revolt of 1929" (unpublished typescript, University of North Carolina, Chapel Hill, 1974); Linda Frankel, "Southern Textile Women: Generations of Struggle," in Karen Brodkin Sacks and Dorothy Remy, eds., *My Troubles Are Going to Have Trouble With Me: Everyday Triumphs of Women Workers* (New Brunswick, N.J., 1984), 39–60; Jacquelyn Dowd Hall, "Women, Kin, and Collective Action: The Elizabethton, Tennessee, Strike of 1929" (unpublished typescript, Woodrow Wilson International Center for Scholars, Washington, D.C., 1985); and Lynn Haessly, "'Mill Mother's Lament': Ella May, Working Women's Militancy, and the 1929 Gaston County Textile Strikes" (unpublished typescript, University of North Carolina, Chapel Hill, 1984).

[64] Tindall, *Emergence of the New South*, 349–50; H. M. Douty, "Labor Unrest in North Carolina, 1932," *Social Forces*, 11 (1933): 580–88; John P. Prior, "From Community to National Unionism: North Carolina Textile Labor Organizations, July, 1932–September, 1934" (M.A. thesis, University of North Carolina, Chapel Hill, 1972), 14–39; John Selby, "Industrial Growth and Workers' Protest," 113–191; and Capus Waynick, interview by Bill Finger, High Point, N.C., November 24, 1974, p. 13.

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In April 1930, fifty-five hundred workers from the Dan River and Riverside Cotton mills marched through the streets of Danville, Virginia, to demonstrate their loyalty to the United Textile Workers of America. Leading the way were the "undesirables," two hundred workers recently fired for union activity. Five months later, Danville mill hands went on strike, protesting wage cuts and the owner's refusal to recognize their union. The trouble in Danville marked the culmination of a great strike wave that spread from East Tennessee through the Carolinas in 1929. Photograph reproduced courtesy of the Schlesinger Library, Radcliffe College, Cambridge, Mass.

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Whether independently organized or union led, each walkout was shaped by local circumstances. But more important than the differences were the experiences strikers shared. In community after community, mill folk turned habits of mutuality and self-help to novel ends. Union relief funds were paltry at best, and survival depended on neighborly sharing. Many of the Marion strikers "had a good garden, and they'd divide their gardens with people that didn't have any." Those who held back found themselves donating anyway. Sam Finley remembered frying chickens for hungry picketers—supplied by a boy who could "get a chicken off the roost and leave the feathers." Baseball games, picnics, and barbecues buoyed spirits and fostered solidarity. As in the routines of daily life, women workers were essential to this mobilization of community resources. "The women done as much as the men," Lillie Price asserted. "They always do in everything."[65]

For the most part, village churches did not support collective action. Yet evangelicalism remained a resource on which rebels could draw. Gathering in an interdenominational tabernacle built for revivals, Elizabethton workers listened to a country preacher warn that "the hand of oppression is growing on our people. . . . You women work for practically nothing. You must come together and say that such things must cease to be." Each night, another crowd "came forward" to take the union oath. Marion activists held "open air services," sang hymns on the picket line, and invited strikebreakers to join the union and be saved. At Greenville's Brandon Mill, strikers drew an analogy between "the cotton mill people" and the "children of Israel [who] were forced to work for the Egyptians," then appealed to "the good Christian people of the city to offer up prayers to God" in the textile workers' behalf.[66]

In these ways, workers fashioned a language of resistance from established cultural forms. But the young people who led the protests had also come of age in a society different from the one their parents had known. Most had grown up in the mill villages or moved as children from the countryside. They did not see themselves as temporary sojourners, ready to beat a retreat to the land, or as displaced farmers for whom "it was heaven to draw a payday, however small." Their identities had been formed in the mill village; they had cast their fate with the mills.[67]

As social stratification increased, men and women who considered themselves cotton mill people traversed the psychic minefields of a changing world. Partic-

[65] Perry Hicks, interview by Sam Howie, 17; Sam Finley, interview by Sam Howie, 21; Lillie Morris Price, interview by Mary Frederickson and Marion Roydhouse, Asheville, N.C., July 22, 1975, pp. 37–38. Also see Haessly,"'Mill Mother's Lament,'" 13–14, 26; and Duane McCracken, *Strike Injunctions in the New South* (Chapel Hill, N.C., 1931), 89.

[66] Knoxville *News Sentinel*, March 14, 1929, p. 1; Vesta and Sam Finley, interview by Mary Frederickson and Marion Roydhouse, Marion, N.C., July 22, 1975, p. 19; and Greenville *News*, April 4, 1929, p. 7. Also see Perry Hicks, interview by Sam Howie, 21. For the classic study of churches and unionism, see Pope, *Millhands and Preachers*. Much needed, however, is further research on the role of independent mill churches and holiness sects in mill village life and labor conflict. For an interesting glimpse, see Federal Council of the Churches of Christ in America, Department of Research and Education, "The Strikes at Marion, North Carolina," *Information Service*, December 8, 1929, pp. 10–12.

[67] Perry Hicks, interview by Sam Howie, 4. For a discussion of the creation of a hereditary population of mill workers, see Rhyne, *Some Cotton Mill People*, 65–77.

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The funeral of balladeer Ella May Wiggins, shot while traveling to a union meeting in Gastonia, N.C., during the 1929 strike. A crowd of mill hands and union organizers watched as Wiggins's five children placed a wreath on their mother's grave. Photograph reproduced courtesy of UPI/ Bettman Newsphotos.

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ularly in urban centers, changes in residential and employment patterns widened the gap between mill and town. Charlotte, for instance, acquired a large population of clerks, service workers, and professionals who settled in the suburbs of Dilworth and Myers Park—physically and socially removed from the mill communities of North and East Charlotte. Hoyle McCorkle remembered the tensions that could result. "The other children would kind of look down on you. You'd go to school and they'd call you a linthead and all that stuff. You was kind of from the wrong side of the tracks." Sam Finley had similar memories, from an adult point of view. Marion merchants "were glad to get your paycheck, glad for you to come up there and spend money in their stores, but to uptown people you were still cotton mill trash."[68]

But such encounters do not tell the whole story. The activists of the 1920s shared a generational experience in more positive ways as well. They had entered the mills during the boom years of World War I, when rising wages and opportunities for promotion gave them reason to hope for better times ahead. Literacy increased with the decline of child labor and the spread of compulsory education; newspapers and magazines brought word of boom times in an affluent, larger world. Perhaps most important was the advent of radio. Recording studios and broadcasters discovered stringband music in the early 1920s and began transforming ballad singing, fiddle playing, and banjo picking into one of America's great popular sounds. Across the Piedmont, mill hands listened to their own music on the Grand Ole Opry and danced to the tunes of local performers playing in the studios of Charlotte's WBT. Eventually homogenized and commercialized as the Nashville sound, country music in the 1920s bolstered a sense of unique, region-wide cultural identity.[69]

New modes of transportation forwarded the same ends, and Model T's figured prominently in the labor upheavals of the 1920s. When the Marion Manufacturing Company started "tightening down on people," Sam Finley, Lawrence Hogan, and their friends drove across the Blue Ridge Mountains to Elizabethton, "hunting somebody to organize them as a union." They brought back Alfred Hoffman, and soon twenty-five-year-old Lawrence Hogan became his right-hand man. Educating himself for "the responsibilities of class leadership," Hogan became a full-time organizer. From his base in Marion, he published *The Shuttle*, a newsletter designed to "carry the message to and fro." Hogan favored the "whirlwind system of distribution. Making about 40 [miles per hour] through a town I toss a bundle of *Shuttles* into the air, the wind whips them away, scatters them, and the mill workers, who have learned to expect them, run out and pick them up." When Hogan died—

[68] Hoyle and Mamie McCorkle, interview by James Leloudis, 5–7; and Sam Finley, interview by Sam Howie, 10.

[69] For insights into the experiences of this post–World War I generation, see Sam Howie, "The New South in the North Carolina Foothills: A Study of the Early Industrial Experience in McDowell County" (M.A. thesis, Appalachian State University, 1978); Harriet L. Herring, "The Metamorphosis of the Docile Worker," in Harriet Herring, *Worker and Public in the Southern Textile Problem* (Greensboro, N.C., 1930), 3–10, and "Industrial Relations in the South and the NIRA," *Social Forces*, 12 (1933): 124–31; *The Charlotte Country Music Story* (Charlotte, N.C., 1985); and Kinney Rorrer, *Rambling Blues: The Life and Songs of Charlie Poole* (London, 1982).

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after a car wreck in 1935—he was mourned as "one of the clearest-eyed leaders of federated workingmen in this section."[70]

Creative tactics, indigenous leaders, lessons in "the power of group action"—these were legacies of the 1920s. Still, victories were isolated and limited by external constraints. The UTW had conceded the deeper sources of workers' discontent. Its policy of "labor-management cooperation" opposed strikes and asked only that workers be made junior partners in the process of rationalization. But even a stronger and more aggressive union would have had difficulty making headway in the region. Southern mill owners monopolized social, economic, and political power. They stood united in their refusal to tolerate even the mildest form of unionism and were strengthened in their resolve by hard times. With the stock market crash of 1929 came massive lay-offs, further wage cuts, and efforts to recoup profits by "stretch[ing]-out the stretch out." The result was an atmosphere of smoldering antagonism. Lawrence Hogan described Piedmont mills in 1932 as "volcano-like. . . . Everywhere you go," he reported, "they are ready to explode."[71]

On June 16, 1933, President Franklin D. Roosevelt signed the National Industrial Recovery Act into law. Introduced with much fanfare, the NIRA promised a "great cooperative movement" among business, government, and labor to bring about national recovery. It relaxed antitrust measures, permitting trade associations to control production and set prices, and encouraged manufacturers to spread employment and stimulate consumer demand by shortening hours and raising wages. Balancing the license given employers' associations was Section 7(a) endorsing workers' right to organize and bargain collectively. The Cotton Textile Institute, formed in 1926 at the instigation of southern manufacturers, welcomed the NIRA and pushed through the first Code of Fair Competition. The code called for a minimum wage of $12.00 per week ($13.00 for northern workers), a forty-hour week, and the prohibition of child labor. Mills throughout the South hoisted the National Recovery Administration's (NRA) Blue Eagle banner, proclaiming "We Do Our Part." Prices, employment, and sales climbed. The industry enjoyed its most profitable year since 1928.[72]

[70] Vesta and Sam Finley, interview by Mary Frederickson and Marion Roydhouse, 17–18; Greensboro *Daily News*, August 25, 1935, p. 4B; and Tess Huff, "A Conference of Southern Workers," *Labor Age*, 21 (1932): 4. We thank John Selby for bringing the article in the Greensboro *Daily News* to our attention.

[71] Herring, "Industrial Relations," 128; Bernstein, *Lean Years*, 97–103; Mrs. B. M. Miller to Hugh Johnson, July 23, 1933, Chadwick-Hoskins Mill, Charlotte, N.C., National Archives, Washington, D.C., Records of the National Recovery Administration, Record Group 9, Records of the Cotton Textile National Industrial Relations Board and the Textile National Industrial Relations Board, Entry 398 (hereafter, NRA Records 398); and Huff, "Conference of Social Workers," 4. For the basic study of the UTW, see Robert R. R. Brooks, "The United Textile Workers of America" (Ph.D. dissertation, Yale University, 1935).

[72] Irving Bernstein, *Turbulent Years: A History of the American Worker, 1933–1941* (Boston, 1970), 30. On the Cotton Textile Institute, see Louis Galambos, *Competition and Cooperation: The Emergence of a National Trade Association* (Baltimore, 1966). For works most helpful to our understanding of early New Deal industrial policies, see William E. Leuchtenburg, *Franklin Roosevelt and the New Deal* (New York, 1963), 55–58, 63–70; Bernstein, *Turbulent Years*, 172–185; James A. Hodges, "The New Deal Labor Policy and the Southern Cotton Textile Industry, 1933–1941" (Ph.D. dissertation, Vanderbilt

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Across the Piedmont, mill hands listened eagerly to Roosevelt's fireside chats and signed on as "members of the NRA." Recovery legislation seemed to place the federal government's imprimatur on ideals of equity, independence, and cooperation. Everyone, from the lowliest sweeper to the most skilled loom fixer, could join in a fervent campaign to put the industry, and the nation, back on its feet. In all this, the New Deal resonated with the past even as it spoke to present needs. But the NIRA also promised something altogether new: the intervention of a powerful third party as leverage against local elites and a guarantor of workers' rights. Within less than a month, union locals had sprung to life in 75 percent of South Carolina's mills. UTW membership jumped from an estimated forty thousand members in September 1933 to two hundred seventy thousand by August 1934. To the shock of government officials and businessmen alike, southern workers organized.[73]

As it turned out, recovery was short-lived. It was one thing for trade associations to endorse the principle of self-regulation, quite another for individual mills to sustain the short-term losses that resulted from lower production and higher labor costs. And mill owners had no intention of joining a "great cooperative movement" with organized labor or surrendering any of their authority in the factories. By the fall of 1933, workers across the country were complaining about bosses who "'chiseled' at the code." Skilled workers watched maximum wages sink toward the minimum, as owners reclassified jobs and cut piecework rates. The benefits of an eight-hour day evaporated, as mill hands found themselves "doing as much in 8 hours as we did in 12." Unionists summarily lost their jobs. Once more, production outran demand, warehouses bulged with unsold goods, and textile workers joined the ragged ranks of the unemployed.[74]

Code chiseling struck hardest at the mills' most vulnerable workers. As long as they could pay low piecework rates, small firms that could not afford new machinery or efficiency experts kept less productive employees on the payroll. Children, older people, pregnant women, mothers bearing the burden of a double workday—such workers might not produce as much as young adults, but they were valued as a pool of cheap labor. Minimum-wage standards changed all that. Hoping to contain the consequent rise in labor costs, mill officials pegged the $12.00 wage to production quotas that only the fastest workers could meet. "They discharged all spinners that could not run 8 sides of spinning and that included women that was the sole dependers [supporters] of their family as bread earners,"

University, 1963); and Theda Skocpol, "Political Response to Capitalist Crisis: Neo-Marxist Theories of the State and the Case of the New Deal," *Politics and Society*, vol. 10, no. 2 (1980): 155–201. On the business upturn of 1933, see Hodges, "New Deal Labor Policy," 196.

[73] Employees of Randolph Mills to Hugh Johnson, February 27, 1934, Randolph Mills, Franklinville, N.C., NRA Records 398; Hodges, "New Deal Labor Policy," 208–09; and Tindall, *Emergence of the New South*, 509.

[74] Bernstein, *Turbulent Years*, 302; and Ralph B. Bowers to Hugh Johnson, February 26, 1934, American Cotton Mills, Bessemer City, N.C., NRA Records 398. Among numerous examples of such complaints, see R. W. Russell to Franklin D. Roosevelt (hereafter, FDR), July 25, 1933, Brown Mill, Concord, N.C.; Noah Scott to Hugh Johnson, October 10, 1933, Aragon-Baldwin Mill, Whitmire, S.C., NRA Records 398.

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wrote one woman. "They have made work so hard [a] woman just can't hold a job," claimed another. To make matters worse, short time and hikes in the cost of living cancelled out the gains enjoyed by those who did manage to hold their jobs at the bottom of the industry's broad job pyramid.[75]

Whether weavers who found their wages cut or spinners who could barely keep up their sides, most who wrote to the NRA spoke not just for themselves but for "my own class of people (textile workers) that are being oppressed so much." And they made clear where they placed the blame: on "low down Boss-men," "these dishonest inhuman industrialists," or, more generally, "the blood thirsty rich." Mill owners could point to numerous bankruptcies and argue, with some justification, that cut-throat competition and slackened demand drove down profits and dictated low wages. Yet, throughout the period, southern mills enjoyed higher rates of return than their northern competitors. The southward trend of the industry reached its peak in 1935, leaving ghost towns across New England where textile mills had thrived. Pressing their advantage through speed ups, consolidation, vertical integration, and continued investment in labor-saving machinery, the South's better-established firms emerged from the Great Depression relatively unscathed.[76]

In any case, the issue for workers was not how wage rates affected profit margins and stockholders' dividends but whether owners were accepting a fair share of the sacrifices demanded by hard times. Mill officials "claim they are not making money," wrote one Gastonia worker whose letter was published in the *Charlotte Observer*. "Well if they are not making money, I ask you, how can they afford to build these fine mansions to live in? How can every member of the family own his or her own car? How can they afford to take trips to foreign countries? How can they afford to send their children to college and obtain the best education? When the poor mill worker can't make enough money to buy milk for his undernourished children . . . and the mothers must watch the flour sack like a hawk watching a chicken to get hold of them to make little under-garments for her children." How could such conditions be changed? A widow from Tapacau, South Carolina, had a modest suggestion: "They could cut down the pay on some of the supers and overseers and president and give to [the] help."[77]

Nothing more vividly revealed the chasm opening between "the common worker and the Capitalist" than the metaphors that laced mill folks' letters to FDR.

[75] W. H. Fowler to Hugh Johnson, July 26, 1933, Appalachee Mill, Greer, S.C.; Ralph B. Bowers to Hugh Johnson, February 26, 1934, American Cotton Mills, Bessemer City, N.C., NRA Records 398; Hodges, "New Deal Labor Policy," 258–59; and John W. Kennedy, "The General Strike of the Textile Industry, September 1934" (M.A. thesis, Duke University, 1947), 154–55.

[76] Greenville *News*, August 2, 1934, p. 4; letter from Mrs. Steele, April 30, 1934, Calvine Mill, Charlotte, N.C., NRA Records 398; Clarence J. Swink to Donald Richberg, March 30, 1935, Cannon Mills, China Grove, N.C., National Archives, Washington, D.C., Records of the National Recovery Administration, Record Group 9, Records of the Cotton Textile National Industrial Relations Board and the Textile National Industrial Relations Board, Entry 402 (hereafter, NRA Records 402); Mrs. J. G. Hutchinson to FDR, December 5, 1933, NRA Records 398; Tindall, *Emergence of the New South*, 361–62; Kennedy, "General Strike," 103; and Hodges, "New Deal Labor Policy," 59, 66–67, 329–56.

[77] Letter to the editor, *Charlotte Observer*, September 13, 1934, p. 4; and Lottie Gainsbrugh to FDR, March 21, 1934, Tacapau Mill, Tacapau, S.C., NRA Records 398.

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One woman reported that the superintendents were "treating the People worse than convicts." "We have been . . . treated worse than beasts," complained another. Above all, workers spoke of "bondage" and "slavery," of "release from our balls and chain." A unionist at Durham's Erwin Mill drew this parallel: "I am glad to know that Abraham Lincoln freed the negroes and am glad to know that now you are trying to free us. White slaves."[78]

Surveillance and economic vulnerability made letter writing an act of enormous courage. In practically every case, workers closed with a plea for anonymity. It would cost them their jobs if mill officials knew what they had done. A woman with a "sick husband and children to support" mailed her letter by train because "the Washington mail is being watched here." A young girl in Whitmire, South Carolina, was also afraid. "If they thought for one instance I had written you they would fire me like lightning." Yet, despite the risk, many offered to step forward and vouch for what they had seen. A. W. Litton from the tiny village of Glencoe wrote, "If Mr green finds out that i rote you this he will fire me and run me off this place but if you can't find out that Mr. W. G. Green is [violating] the N.R.A. law in every way call for me and i will show what is going on here."[79]

Such men and women saw federal intervention as a two-way street. They took the law at face value and coupled petitions for help with promises of responsible citizenship and political support. "The laboring people here are trying to uphold our President and also the code. We believe in it—We talk it—And we would so love to live it," wrote Mrs. B. M. Miller from Charlotte. "Dear Demcrate Friend, I am trying to provide [abide] by the code, but our country is not. Our overseer don't want us to no anything about the NRA code . . . but I am going to provide by it even if I do loss my job," pledged a man in Bessemer City, North Carolina. "If we get fully organized we are all going to vote and elect you again," promised a Durham unionist, "for you are the only [president] that [was] ever for the working man."[80]

This outpouring of faith, anger, and fear was shunted through a bureaucratic maze. Disputes in other industries were referred to the National Labor Board, which, despite its inability to force compliance, began hammering out enduring principles of labor law. By contrast, textile workers' complaints wound their way

[78] J. Vernon Phillips to FDR, September 13, 1934, NRA Records 402, Miscellaneous Cases Closed; Dorothy Taylor to FDR, February 14, 1934, American Spinning Company, Greenville, S.C., NRA Records 398; Mary Alice Ellenburgh to FDR, September 1934, Lonsdale Mill, S.C., NRA Records 402; anonymous letter to FDR, September 10, 1934, National Archives, Washington, D.C., Records of the National Recovery Administration, Record Group 9, Records of the Cotton Textile National Industrial Relations Board and the Textile National Industrial Relations Board, Entry 401, Complaints of Workers—Individuals (hereafter, NRA Records 401); and Archie L. Dunn to FDR, June 7, 1934, Erwin Mills, West Durham, N.C., NRA Records 398. For other uses of the slavery metaphor, see Mrs. M. G. Hunter to FDR, n.d., Louise Cotton Mill, Charlotte, N.C., NRA Records 402; and E. M. Flowers to Hugh Johnson, October 27, 1933, Priscilla Mill, Gastonia, N.C., NRA Records 398.

[79] Mrs. Hamilton to FDR, September 1, 1933, Iceman Mill, McCall, S.C.; Inez Broome to Hugh Johnson, October 25, 1933, Aragon-Baldwin Mill, Whitmire, S.C.; A. W. Litton to Hugh Johnson, August 20, 1934, Glencoe Cotton Mill, Burlington, N.C., NRA Records 398.

[80] Mrs. B. M. Miller to Frances Perkins, December 13, 1933, Chadwick-Hoskins Mill, Charlotte, N.C.; Fritz Howell to Robert Bruere, March 10, 1934, American Spinning Mill, Bessemer City, N.C.; Archie L. Dunn to FDR, June 7, 1934, Erwin Mills, West Durham, N.C., NRA Records 398.

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through local, state, and district committees until they reached the Cotton Textile National Industrial Relations Board, which followed the extraordinary procedure of forwarding them to agents of the industry's trade association, the Cotton Textile Institute. Investigators then contacted the complainant's employer. If the mill denied wrongdoing, the case was closed. Between August 8, 1933, and August 8, 1934, the Board received 3,920 complaints. It authorized ninety-six investigations and resolved only one wage and hour dispute in a worker's favor. As far as can be determined, the board never ordered reinstatement of a worker fired for union activity or even held a hearing in such a case.[81]

The shortcomings of this system were not lost on mill folk. "If Mr. Sloan [head of the CTI] thinks that mill workers in S[outh] C[arolina] are satisfied with their wages," advised one worker, "he ought to investigate among the workers and not among the mill officials and office force." A weaver at Charlotte's Chadwick-Hoskins Mill objected to the practice of appointing "big mill men" to the textile boards. "They are not going to turn in . . . any kind of report which would . . . mean that they would have to pay out more money from their own pockets. The thing . . . to do, is to appoint some good, Honest, Hard working man, say one from each mill community, so he . . . could get the true fact of this stretch out system." When Mrs. Miller of Charlotte was notified—for the third time—that she had failed to reach "the right board," she wrote in exasperation to Secretary of Labor Frances Perkins. "Now if there is a right board I, as well as all the Laboring people of Charlotte would like to know where it is. . . . We have just lots of good citizens in Textile Plants, but we cant come out of bondage alone. We must have help. We must have some one to breake the shackles. We need action now. If we don't get it, it will lead to strikes. WHO WILL BE TO BLAME? NOT THE LABORING PEOPLE. BUT THE PEOPLE WHO HAS POWER TO HELP US NOW."[82]

When help was not forthcoming, mill workers took matters into their own hands. In June 1934, the textile board ignored the suffering already caused by short time, unemployment, and rising living costs and attacked the problem of overproduction with a 25 percent cut in machine hours. Many mills simply shut down every fourth week, reducing wages accordingly. Local unionists in northern Alabama struck back on July 14, pulling twenty thousand workers out of the mills. They demanded a $12.00 minimum wage for a thirty-hour week, abolition of the stretch-out, reinstatement of workers fired for union activity, and union recognition. On August 14, a national UTW convention dominated by militant southern representatives called for a general strike of the textile industry to begin at midnight on Saturday, September 1. Union vice-president Francis Gorman took on the formidable task of organizing hundreds of walkouts in widely scattered mills. Workers organized into "flying squadrons" sped through the Carolinas,

[81] Bernstein, *Turbulent Years*, 172–73, 302–04; and Hodges, "New Deal Labor Policy," 253–54. The Cotton Textile National Industrial Board gained exclusive jurisdiction primarily because of the UTW's weakness and the textile owners' political clout; Bernstein, *Turbulent Years*, 300–04.

[82] M. J. James to Francis Gorman, March 24, 1934, Alice Cotton Mill, Easley, S.C.; Frank Snipes to FDR, August 4, 1933, Chadwick-Hoskins Mill, Charlotte, N.C., NRA Records 398; and Mrs. B. M. Miller to Frances Perkins, December 13, 1934, Chadwick-Hoskins Mill, NRA Records 402.

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forcing plants to close "so rapidly that tabulators almost lost check." By the end of the week some four hundred thousand employees had deserted the mills, closing down the entire industry.[83]

Governors were immediately swamped with demands for military intervention. Twenty-two Gaston County mill executives signed a petition spelling out their fears.

> Several times daily hundreds of men, women and children parade through town yelling, screaming, threatening and intimidating citizens of the town, stating they intend confiscating automobiles and other personal property as their needs demand. . . . Our situation is desperate, much more desperate than we can express in words. Law and order no longer exists, except for the surrender of personal and property rights by a majority of the substantial citizenship and unless something is done within the next twenty-four hours no authority can be held responsible for results.

By September 7, fourteen thousand troops were on duty in the Carolinas alone. A week later, Governor Eugene Talmadge of Georgia declared martial law and interned flying squadron participants in a makeshift "Detention Camp." To panicked townsmen, the strike represented "the gravest emergency which has confronted our people since Reconstruction Days."[84]

The depth of strike support, and the violence of repression, varied from community to community. The Chiquola Mill in Honea Path, South Carolina, was divided "about half and half" between union and nonunion workers. When flying squadrons from nearby villages encircled the plant, local authorities passed out picker sticks and deputized "just about [anybody who] could carry a gun." The result was "a regular riot" in which seven strikers died. By contrast, the General Strike in Durham, North Carolina, was peaceful, unanimous, and self-contained.[85] Individual responses were even more complex. Some workers joined with passionate conviction. Others saw the flying squadrons as "a disorganized mob" and dreaded the divisiveness unions might bring. A Greenville woman spoke for those whose nonparticipation did not imply consent. "I like thousands of others did not strike with the union, because we knew we would loose our jobs like thousands of others have done before us. Even though we might have had faith in the union we simply couldn't afford to quit because we live right up to every penny we make. . . . It is true that every textile worker in the south would walk out of the mill to day if they were not afraid of starvation. I don't *believe* that God

[83] Bernstein, *Turbulent Years*, 305–06; Raleigh *News and Observer*, September 6, 1934, p. 2; and Kennedy, "General Strike," 54.

[84] Petition to Governor J. C. B. Eringhaus, September 5, 1934, North Carolina Department of Archives and History, Raleigh, North Carolina, Governor J. C. B. Eringhaus Papers, box 103; Benjamin B. Gossett to Governor Eringhaus, September 14, 1934, *ibid.*, box 104; and J. M. Zimmerman to Eugene Talmadge, January 14, 1935, NRA Records 402. On the use of troops in the Carolinas and Georgia, see Hodges, "New Deal Labor Policy," 281; and Tindall, *Emergence of the New South*, 511.

[85] Mack Duncan, interview by Allen Tullos, 13, 15–16; and Janiewski, *Sisterhood Denied*, 3–4. A picker stick was a heavy wooden dowel around which cotton was wound before carding.

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intended people to suffer as we have suffered. . . . The life of the average textile worker is a tragic thing."[86]

The General Strike, whatever else it may have been, was a moment in history that laid bare longings and antagonisms ordinarily silenced, distorted, and repressed. Cotton mill people in the 1930s did not subscribe to an abstract, universalistic notion of class solidarity. If nothing else, deep racial divisions militated against such perceptions. But mill hands did see themselves as a people apart, exploited by men with interests opposed to their own and denied opportunities for progress that had seemed within their grasp.[87] The ten thousand mourners who converged on the funeral of the strikers killed at Honea Path, the hundreds of young men and women who climbed into "Fords and rush[ed] across counties to join other familiar-unfamiliar young people in clamoring at the mesh wire of mill gates," the workers in Durham who denied executives access to their own mills—these people broke through the restraints imposed by political isolation and economic defenselessness to make their mark on the times.[88]

Still, the opposition held all the cards. Mill owners whose warehouses were already stocked to overflowing with unsold goods simply "stopped off" and waited out the conflict. Picket lines faltered before machine guns and fixed bayonets. The UTW could not begin to provide adequate strike relief. Nor could mill folk hope for local backing in a region thoroughly dominated by conservative political elites. And Franklin Roosevelt, on whom southern workers relied, "either could not or would not grasp the critical importance" of unionization. Always more interested in agricultural policy than the problems of industry, and dependent on southern congressional support for passage of New Deal legislation, Roosevelt refused to intervene. Keeping his distance, he appointed a mediation board, headed by New Hampshire governor John G. Winant, which found merit in labor's grievances but recommended little more than further study. The president added his personal plea to the board's request that the union end the strike and that employers take back striking workers. Afraid of losing all government support, the UTW

[86] Roy Lee and Mary Ruth Auton, interview by Jacquelyn Hall, 7–11; and anonymous letter to FDR, September 10, 1934, NRA Records 401.

[87] Particularly relevant here is the debate over traditional notions of class consciousness. See, for example, Harry Boyte, "Populism and the Left," *Democracy*, 1 (1981): 53–66; Craig Calhoun, *The Question of Class Struggle: Social Foundations of Popular Radicalism during the Industrial Revolution* (Chicago, 1982); and James R. Green, "Populism, Socialism, and the Promise of Democracy," *Radical History Review*, 24 (1980): 7–40. Racial antipathy was notably absent from white mill workers' letters to Roosevelt and the NRA. In fact, a number of complainants explicitly included blacks among "the people" for whom they spoke. See, for example, Fritz Howell to Robert Bruere, March 10, 1934, American Spinning Mill, Bessemer City, N.C., NRA Records 398. All in all, white workers in the 1930s were much less concerned with racial ideology than with the more immediate and pressing injuries of class bias. For a discussion of heightened perceptions of class inequality in Depression-era America, see Robert S. McElvaine, *Down and Out in the Great Depression: Letters From the Forgotten Man* (Chapel Hill, N.C., 1983), 8–13.

[88] Bernstein, *Turbulent Years*, 310; Jonathan Daniels, *A Southerner Discovers the South* (New York, 1938), 26; and Janiewski, *Sisterhood Denied*, 3. Also see T. J. Jackson Lears, "The Concept of Cultural Hegemony: Problems and Possibilities," *AHR*, 90 (1985): 567–93.

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complied. The General Strike officially ended on September 22, twenty-two days after it began.[89]

In hindsight, Francis Gorman's poststrike claim that the Winant board's report constituted an "overwhelming victory" for the UTW appears patently absurd. But to mill folk at the time it seemed more plausible. Only gradually, in the face of evictions and blacklisting on a massive scale, did a different interpretation emerge. The letters that continued to pour into the White House indicated not a quick fading away of local unions but valiant attempts to hold out in the face of overwhelming odds. Returning to the Matthews Mill in Greenwood, South Carolina, workers confronted a ten-week lockout. "It look like the owner of this mill is doing every thing he can to tare our union up, but I feel like you will help us in some way where we can hole our people to-geather for our rights," wrote Ruby Brown. A coworker added her testimony: "When the people here went back in to their jobs, many of them were discharge . . . because they joined a union. . . . [T]he managers by discharging our strongest union members therefore broke up our local union, causing the rest of us to submit to them to hold a job." From across the Piedmont came similar stories of employers "break[ing] up our union," of workers forced to "submit."[90]

Eviction was the employers' ultimate sanction. Nothing produced more terror than the prospect of being out on the road with "nowhere to go" and "cold weather . . . coming on." A man in Williamston, South Carolina, lost his job for being elected president of his union local. After the General Strike, his son and daughter were discharged as well. "Then they forced me out of the house i was living in i had no other place to go. . . . I have tried in over 300 cotton mills for work and they will not hire me because I belong to the union. . . . There is starvation ahead if i don't get work at *once*. . . . Please for God's sake help me."[91]

We can only speculate about what might have happened had the General Strike occurred later, after the Wagner Act had strengthened labor's right to organize and the UTW had joined forces with the more aggressive Congress of Industrial Organizations (CIO).[92] The strike itself might have ended as it did, but with less

[89] Edward and Mary Harrington, interview by Mary Murphy, Burlington, N.C., February 28, 1979, p. 11; Leuchtenburg, *Franklin Roosevelt*, 108; and Bernstein, *Turbulent Years*, 313–34. For a discussion of the weakness of labor's allies within the region, see Alan Brinkley, "The New Deal and Southern Politics," in James C. Cobb and Michael V. Vamorato, eds., *The New Deal and the South* (Jackson, Miss., 1984), 97–115.

[90] Bernstein, *Turbulent Years*, 314; and Ruby Brown to FDR, December 3, 1934, Matthews Mill, Greenwood, S.C.; Mrs. J. G. Minor to FDR, November 5, 1934, Greenwood Cotton Mill, Greenwood, S.C.; Mrs. Kenneth Sims to FDR, November 20, 1934, Worth Spinning Company, Stony Point, N.C., NRA Records 402. Hodges estimated that between fifty thousand and one hundred thousand people were unable to regain their jobs. Between September 1934 and September 1935, the Textile Labor Relations Board (which replaced the discredited industry-dominated board) received 4,374 complaints indicating that prestrike practices were continuing, while lockouts and wholesale firings were additional threats that workers faced during the bleakest months of the Depression. See Hodges, "New Deal Labor Policy," 302, 314.

[91] Milan Owens to FDR, October 10, 1934, Ware Shoals Manufacturing Company, Ware Shoals, S.C.; W. A. Davis to FDR, November 8, 1934, Matthews Mill, Greenwood, S.C.; B. E. Brookshire to FDR, November 25, 1935, Laurens Cotton Mill, Laurens, S.C., NRA Records 402.

[92] The NIRA was struck down by the Supreme Court in 1935. It was replaced by legislation sponsored by Senator Robert F. Wagner of New York that strengthened Section 7(a), which had been ineffectual under the NRA. For two perspectives on how Wagner's initiatives combined with

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devastating effects. As it was, working-class leaders—those who had seized the opportunities for political participation and collective action offered by the New Deal—were purged from the mills, while those who stayed suffered a debilitating erosion of faith and will.

The secretary of Local 2265 in Kannapolis, North Carolina, writing to Francis Gorman, made these feelings clear. "Our local is gone and it dont seem there is any use to try now as they have lost faith in the union. We have had so many promises and nothing done. I my self am almost ready to give up. . . . What is wrong[?] [H]ave the whole works sold out[?] . . . We as a poor hungry people cannot live with[out] something to eat and something to wear and to keep us warm. How do you people in Washington think we can go on living on air and promises. What we need is help and if you cannot get that for us then say so and we will not depend on promises any longer. . . . [I]t looks like Cannon Mills are running the whole thing. We want to know if they run the whole country. It looks like it. . . . Please . . . do something that we may be able to still have faith in our government."[93]

Mill hands learned from their history, and in 1934 the lesson for many was a deep distrust of government and trade unions alike. Above all, the General Strike drove home the cost of challenging the established order. Better the familiar securities of job and home than "air and promises," followed by exile, suffering, and defeat.[94]

SOUTHERN WORKERS DID NOT ABANDON efforts to shift the balance of power in the mills. Their disillusionment with the NRA helped spur passage of the Wagner Act in 1935. Heartened by this restructuring of labor law, the CIO launched a southern organizing drive; by the end of World War II textile unionism had established a small but permanent beachhead in the region. Still, labor's spectacular gains in other industries bypassed the textile South. Southern legislators, unhindered by working-class bargaining power, led a postwar attack on labor's legislative gains. The unorganized South remained a mecca for runaway shops and an ongoing source of cheap labor.[95]

Within this environment, textile leaders enlarged on the strategies developed in the 1920s and 1930s. To earlier forms of rationalization, they added destruction of the communities they had created and tried so hard to subdue. After 1938, when the Fair Labor Standards Act narrowed the wage differential between North and

working-class pressures to strengthen New Deal labor policies, see Skocpol, "Political Response," 179–201; and Samuel P. Hays, "The New Deal: After Fifty Years" (unpublished typescript, University of Pittsburgh, 1985), 3–10.

[93] G. W. McElroy to Francis Gorman, November 1, 1934, Cannon Mill, Kannapolis, N.C., NRA Records 402.

[94] For a similar point about the legacy of repeated setbacks in the democratic movements of the late nineteenth century, see Escott, *Many Excellent People*, 263–66.

[95] Tindall, *Emergence of the New South*, 517–21; and F. Ray Marshall, *Labor in the South* (Cambridge, Mass., 1967), 246–69, 324–29.

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South, the maintenance of company housing became an economic disadvantage. Burlington Mills began selling its villages in the wake of the General Strike, and other firms followed suit. With the aid of improvements in highway transportation, which they had helped promote, southern mill owners gradually dismantled their villages, hired workers from the surrounding countryside, or relocated in rural industrial parks.[96]

The retired workers we talked to welcomed rising wages and diminution of company control, but they reported the unraveling of social relations as a personal loss. Work was severed from community life. People "didn't visit like they once did," and neighbors seemed distant and aloof. "People misses a lot by not having community," Mary Thompson explained. "I believe it made you more secure or something. But now you're scattered. You work maybe one place, and another works way over yonder, and you don't get close to nobody."[97]

At least some manufacturers saw the dispersion of people and the dismantling of neighborhoods in a more favorable light. A North Carolina textile promoter explained: "[Our] goal was to have one industrial employee from every farm family in [the county]. We stressed this continually until I think it got to the point that people believed it, and the employment practices of the mills we brought here confirmed it. They found that by scattering their labor they were never available in large enough numbers to attract the union. When you get a lot of people living in one community, living in one mill village, they're naturally objects of concern—and, of course, exploitation—by labor unions. But scattering these people out all over the country turned out to be a very healthy concept."[98]

Thus was a world of cotton mill people made and unmade. Pushed off the land, white farmers created a mill village culture that sustained their personal lives and public protest. They suffered poverty and exploitation, but they did not live in a closed society that stripped them of independence, hope, and dignity. On the contrary, the manufacturers' authority was hedged about by technological and social constraints. The assault on the mill workers' world, which began with welfare work and culminated in owners' efforts to bend New Deal legislation to their own advantage, sparked the largest single strike in American history. The men and women who came forward in unparalleled numbers in 1934 did so in the belief that powerful allies were by their sides. Far from silent or incoherent, they may have misread the dynamics of national politics, but they understood the realities of power. "It is a great struggle for the people who are not so fortunate as to possess

[96] Harriet L. Herring, *Passing of the Mill Village: Revolution in a Southern Institution* (Chapel Hill, N.C., 1949), 8–23, 128–31; Triplette, "One-Industry Towns," 72–137, 225–34; and Chip Hughes, "A New Twist for Textiles," in Marc S. Miller, ed., *Working Lives: The Southern Exposure History of Labor in the South* (New York, 1980), 338–51.

[97] Carl and Mary Thompson, interview by James Leloudis, 47–48. Also see Glass, "Southern Mill Hills."

[98] Raymond Shute, interview by Wayne Durrill, Monroe, N.C., June 25, 1982, pp. 56–57. Also see Caesar Cone to Harriet Herring, "Long Range Program for Modernization of Cone Mills' Greensboro Villages," March 1, 1950, Louis R. Wilson Library, University of North Carolina, Chapel Hill, Southern Historical Collection, Harriet Herring Papers.

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the requirements that is necessary to make Liberty," wrote a mill hand in the aftermath of the 1934 conflict. "Those who are at Liberty and Freedom *now* are those who own and possess authority and money." The defeat of the General Strike, the subsequent failure of unionization, and the breakup of the villages ended an era and reshaped the terrain of labor conflict in the postwar South.[99]

[99] Mrs. Ralph Cullom to FDR, November 17, 1934, Rosemary Manufacturing Company, Roanoke Rapids, N.C., NRA Records 402.

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Atrocious Misery: The African Origins of Famine in Northern Somalia, 1839–1884

WAYNE K. DURRILL

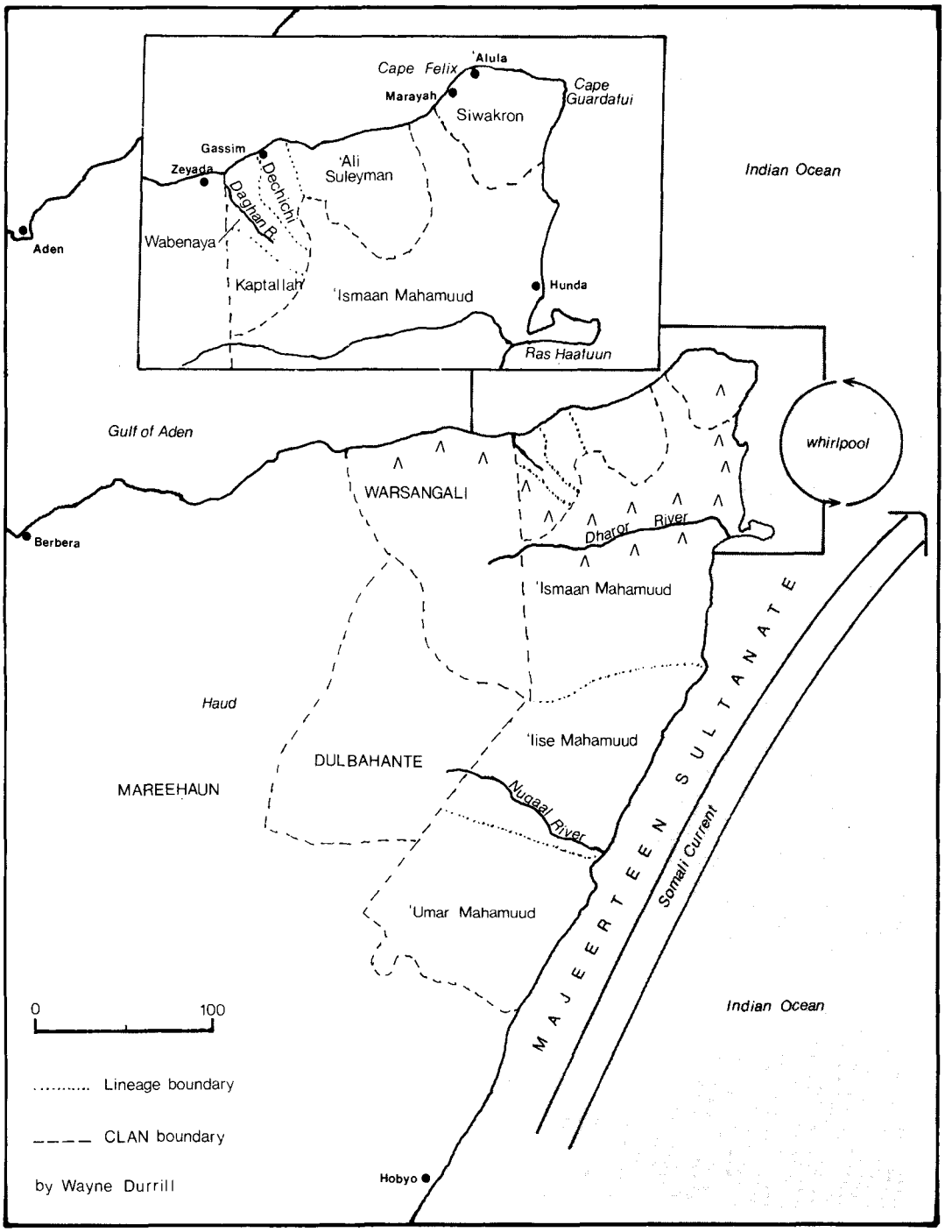
ON JUNE 7, 1801, AN EAST INDIA COMPANY FRIGATE, the *Weisshelm*, sailing from India to Egypt, approached Africa's east coast near the Horn. When, at seven in the evening, the ship drifted close to the rocky shore, it foundered, broke in two, and lodged between two boulders. The next morning, the *Weisshelm* survivors found themselves on a beach surrounded by perhaps twenty Africans "armed, some with matchlocks, and some with bows and arrows, but all carried a large knife like a sabre, and pike." The Africans stripped the castaways of their clothing and other possessions. They also removed silver bracelets from the officers' Indian servants by first cutting off their arms. Toward sunset, a local chief approached the beach with more soldiers. He spoke awhile with his men, then formed them into a line, and ordered the warriors to brandish their weapons at the Europeans "as if they were going to kill" them. The *Weisshelm*'s survivors made a dash for some nearby hills. Those "who could not run fast enough, or were wounded by [the Africans'] shots, were immediately butchered, as soon as overtaken." Those who escaped were rescued some weeks later by a British brig-of-war that became becalmed close to the Somali shore.[1]

The *Weisshelm*'s crew and passengers were stranded near a promontory called Ras Ḥaafuun in what is now northern Somalia. Their experience was not unique. They ran afoul of the Majeerteen, a group infamous among European sailors for their fearsome treatment of castaways. The Majeerteen alone among Africans systematically scavenged among the shipwrecks that regularly littered their shores. These wrecks were caused by strong currents that swept northward in July and August at seven to eight knots off the Somali south coast. The currents flowed northeast to Ras Ḥaafuun where they turned directly east, forming on the

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[1] *Narrative: Portenger's Shipwreck in the Red Sea* (n.p., n.d.), 3–24. I used the copy, which was bound as a pamphlet, at the Duke University Library, Durham, North Carolina. Somalia only recently adopted a standard orthography for its language, and that guide has not yet been translated into English. Therefore, I have followed the orthography used by Said Samatar in his recent work on northern Somalia, including his modifications for the convenience of English readers. I could not find a few words, especially place names, rendered in the new orthography. In such cases, I have retained spellings found in nineteenth-century sources. Said S. Samatar, *Oral Poetry and Somali Nationalism: The Case of Sayyid Mahammad 'Abdille' Hasan* (New York, 1982), viii.

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MAP: Horn of Africa, Mid-Nineteenth Century.

SOURCES: I. M. Lewis, *Peoples of the Horn of Africa: Somali, Afar, and Saho* (London, 1955); George Revoil, *La Vallée du Darror* (Paris, 1882); L. Bricchetti Robecchi, "Da Obbia ad Allula," *Bullettino della Società Geographica Italiana*, ser. 3, vol. 4 (1891); and Giovanni de Agostini, *La Migiurtina ed il Territorio del Nogàl* (Turin, 1926?).

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currents' backside an enormous whirlpool rotating counterclockwise toward the shore. Ships caught in the whirlpool were carried westward to the rocky coast between Ras Ḥaafuun and 'Alula on the point of the Horn.[2]

By 1800 the Majeerteen confidently expected two or three European ships to be wrecked on their shores each season. When that happened, nearby residents converged on the site, chased away the survivors, and looted the vessel. As early as 1800, booty provided the means by which local Majeerteen chiefs assured themselves of political power. They supervised the sale of loot in Arabia and distributed half of the proceeds to their kinsmen—now clients—thus creating obligations that could be exchanged later for rights in labor, water, and the use of pastures. By about 1840, however, that patronage and wealth had become concentrated in the hands of a single lineage of herders, the 'Ismaan Maḥamuud, who organized the sole sultanate among the northern Somali. So important did shipwrecks become to the sultanate that in 1878 an American visitor among the Majeerteen reported: "A priest is stationed in the mountains near Cape Guardafui who prays day and night that God will drive Christian vessels ashore that they may plunder them! This was told me by the Chief of Hunda who regarded it as a very prudent, proper and pious precaution—he thinking I was a Moslem."[3]

Booty from shipwrecks and the patronage it generated transformed the Majeerteen political economy. In the early nineteenth century, Majeerteen herders and fishermen organized themselves into small local groups that produced a subsistence, if little else, despite a scarcity of natural resources. The mobility of the herders enabled them to exploit pastures over a wide area and to harvest gum for sale, which tided them over the dry season. They also developed an interdependent relationship with coastal peoples whose labor shortages often complimented the herders' labor surpluses. Finally, the Majeerteen practiced conservation in times of drought. In each of these strategies, the Majeerteen recognized the limitations of their resources. They used their knowledge of the country and deployed the labor they controlled to take best advantage of what the region had to offer. In consequence, when the rains failed, no one died of

[2] J. G. Bruce, "Somali Current: Recent Measurements during the Southwest Monsoon," *Science*, July 1, 1977, pp. 51–53; and Friedrich Schott, "Monsoon Response of the Somali Current and Associated Upwelling," *Progress in Oceanography*, 12 (1983): 357–81. For information on the frequency of wrecks on the Majeerteen shores, see F. M. Hunter, *An Account of the British Settlement of Aden in Arabia* (1877; reprint edn., London, 1968). Hunter recorded seven British shipwrecks on the Majeerteen coast between June 1870 and July 1874. A rate of two shipwrecks per year coincides with a report by Charles Graves, whom Khedive Ismaa'iil assigned to lead a naval expedition to survey the Somali coast. In 1878, villagers near Cape Guardafui told him that they "confidently expect[ed] two or three steamers to be wrecked this summer." Charles Graves to Chichi Graves [his wife], May 22, 1878, University of North Carolina Library, Chapel Hill, Southern Historical Collection, Charles I. Graves Papers (hereafter, Graves Papers), p. 32; and Hunter, *Account of the British Settlement of Aden*, 178–80. Graves wrote this forty-page letter as rough notes for a report that he later sent to the Egyptian army. I used a typescript on microfilm of Graves's letter, which has page numbers added. The original letter is also available in the Southern Historial Collection. On Graves's report, see Pierre Crabitès, *Ismail: The Maligned Khedive* (London, 1933), 140, and *Americans in the Egyptian Army* (London, 1938), 230. For information on the Graves papers, see Wayne K. Durrill, "African Papers in the Southern Historical Collection, The University of North Carolina Library at Chapel Hill," *History in Africa: A Journal of Method*, 7 (1980): 337–42.

[3] Charles Graves to Chichi Graves, May 22, 1878, Graves Papers, p. 21.

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starvation, and no general warfare ensued. After 1840, the 'Ismaan sultanate, however, sought profit, not merely subsistence, through its participation in an overseas commodities market. A new regional political structure built out of patronage and derived from the distribution of loot from shipwrecks enabled the 'Ismaan Maḥamuud clan to alter local production and to expand Majeerteen trade. This political economy produced an unequal but rising prosperity for most Majeerteen people, hence a constituency for 'Ismaan rule and for trade in external markets. But the 'Ismaan sultanate's policies also transformed periodic regional droughts from manageable hardships into catastrophic famines.

IN THE NINETEENTH CENTURY, the Majeerteen clan occupied a rocky, semidesert territory on the Horn of Africa, a strip of land perhaps one hundred fifty miles wide and four hundred miles long. The Majeerteen numbered twenty to thirty thousand persons, divided among three major lineages—the 'Ismaan Maḥamuud in the north, the 'Iise Maḥamuud in the middle, and the 'Umar Maḥamuud in the south. The Majeerteen also permitted two distantly related lineages to live in their midst—the Siwakron and the 'Ali Suleymaan, both of which resided near Cape Guardafui along the northern part of the coast where shipwrecks occurred.[4] The Warsangali, Ḍulbahante, and Mareeḥaun clans occupied areas west and south of the Majeerteen country. Those three clans plus the Majeerteen formed the Harti clan confederation.

Majeerteen clansmen claimed membership in two basic social groups—a family and a sublineage. A sublineage was a mag-paying group, that is, a group that collected a portion—mag—from each member's annual produce for common use by the sublineage. Sublineages formed the Majeerteen's fundamental political units. Each sublineage consisted of several patrilineally related families that together took responsibility for all the actions of the sublineage's members. In the event of transgression or dispute within or without the group, sublineage leaders negotiated a settlement with the wronged party or, failing that, engaged in a blood feud. A European traveler, however, reported that "blood feuds are infrequent, commutation by fine generally being preferred, and are carefully avoided if possible." To pay such indemnities, the group pooled the mags collected from each member. Families formed the Majeerteen's basic economic units. Women brought property to a marriage, usually a dowry of one hundred fifty Maria Theresa dollars, large palm mats used to build a hut, and furnishings. Men brought their alliances with other men in the Majeerteen's patrilineal kin system, hence, access

[4] Charles J. Cruttenden, "Report on the Mijjertheyn Tribe of Somallies, inhabiting the district forming the North-East point of Africa," *Transactions of the Bombay Geographical Society*, 7 (1844–46): 116. On the settlement of the Horn, see David Hamilton, "Imperialism Ancient and Modern: A Study of British Attitudes to the Claims to Sovereignty to the Northern Somali Coastline," *Journal of Ethiopian Studies*, 5 (1967): 11; I. M. Lewis, "The Somali Conquest of the Horn of Africa," *Journal of African History*, 1 (1960): 213–29; and Mohamed Nuuh Ali, "A Linguistic Outline of Early Somali History," *Ufahamu*, 12 (1983): 234–42. For an appraisal of studies on early Somali history, see Herbert S. Lewis, "The Origins of the Galla and Somali," *Journal of African History*, 7 (1966): 27–46; and Lee V. Cassanelli, *The Shaping of Somali Society: Reconstructing the History of a Pastoral People, 1600–1900* (Philadelphia, 1982), 28–37.

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to livestock, wells, pastures, protection, and relief in hard times. Men and women labored in complementary occupations. Wives reared children, cooked, fetched wood and water, and tended livestock close to home. Husbands herded livestock miles away, hunted, collected gum, defended control of pastures and wells, and traded with coastal merchants. Husbands controlled the disposition of resources and labor. Intransigent wives found themselves quickly and easily divorced and returned to their fathers' homes.

Each Majeerteen person also claimed rights and protection from his own lineage, clan, and clan confederation, each of which served mainly as an intermediary among contentious clansmen. Herding lineages defined and defended their members' pasturing rights against claims lodged by competing lineages. Coastal lineages controlled access to export markets and provided an *abbaans* (protector) for Indian and Arab merchants, who dominated the export trade in the early nineteenth century. At a higher level, the Majeerteen clan mediated disputes among its own lineages in a council of elders that met each year. And the Harti clan confederation circulated women and their dowries among the four clans. In short, power lay in the hands of local leaders—family heads and sublineage chiefs. Only in exceptional circumstances did the lineage, clan, or clan confederation wield any influence in the Majeerteen country.[5]

Before 1839, local Majeerteen leaders directed distinctly local economies in which each Majeerteen family produced a modest but adequate living from scanty natural resources scattered among three distinct ecological zones. Limestone hills and plains in the interior formed one zone. These dry uplands, the Haud, produced enough grass to support camels and goats but little else. The largest of the plains lay in the Dharor Valley, ordinarily occupied by 'Ismaan herders. Rugged marble hills and cliffs that paralleled the coast formed a second zone. Rock crevices there provided a unique micro-environment for acacia trees from which Majeerteen herders harvested gum arabic and frankincense in the dry season. Finally, the sea and its shore formed a third area that offered a better, though still limited, variety of natural resources—fish from the ocean, a fruit stone used to make coarse bread, wild palm trees that produced dates and palm leaves used to make matting, and grassy uplands that fed scattered herds of both domestic livestock and wild game, especially antelope.[6]

[5] Cruttenden, "Report on the Mijjertheyn," 118–21. There are no scholarly studies of either the Majeerteen or their history. Several works on other Somali clans, however, cover the northern region of the Horn. See I. M. Lewis, *A Pastoral Democracy: A Study of Pastoralism and Politics among the Northern Somali of the Horn of Africa* (London, 1961), *Marriage and the Family in Northern Somaliland* (Kampala, Uganda, 1962), *The Modern History of Somaliland: From Nation to State* (New York, 1965), and Samatar, *Oral Poetry and Somali Nationalism.*

[6] Cruttenden, "Report on the Mijjertheyn," 112–13; and Samuel Barrett Miles, "On the Neighbourhood of Bunder Marayah," *Journal of the Royal Geographical Society*, 42 (1872): 61–63. Cruttenden and Miles both wrote detailed reports of production and trade on the northern Somali coast. They were assistant political agents at Aden in charge of procuring food for the town. Therefore, both made regular and lengthy trips to Berbera and the Majeerteen country. For information on the physical features and climate of the Majeerteen country, see Great Britain, War Office, General Staff, *Military Report on Somaliland, 1907*, 2 vols. (London, 1907), 1: 21–44 (hereafter, *Military Report on Somaliland*); I. M. Lewis, *Peoples of the Horn of Africa: Somali, Afar, and Saho* (London, 1955), 56–66; C. F. Hemming, "Vegetation of the Northern Region of the Somali Republic," *Proceedings of the Linnean*

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The main problem in each of these areas was a scarcity of water. The uplands lacked sufficient moisture to provide drinking water or to grow grass except in patches shaped like a crescent moon (called "vegetation arcs" in recent botanical literature). Majeerteen herders therefore ordinarily dispersed their livestock and migrated from one range to another by way of clan-owned wells, rarely staying more than three weeks in one place. They remained far inland during the hot season, August through November. After the northeast monsoons in September, they moved their herds to lower plains, where the grass had been regenerated. Herders ordinarily relied on milk from their livestock as their principal source of food. In the hot season, however, goats and camels stopped giving milk. Majeerteen herders therefore harvested gum from wild acacias in the coastal hills to sell for rice imported from India. Meanwhile, women and children tended the livestock brought from the plains and now pastured at the clan's permanent wells near the coast. In this way, Majeerteen pastoralists made use of their herding economy's flexibility, its main advantage.[7]

Even along the coast, where resources were greater, the Majeerteen seldom produced a surplus of foodstuffs in a year's time. But the coast did provide the fishing peoples with some goods for trade. The residents of Hunda, a tiny village on the south coast, shipped small amounts of frankincense, shark skins, ambergris, palm matting, and ivory to Arabia and India. Coastal residents, however, did not have easy access to drinking water because they lived on islands surrounded by salt marshes. The marshes probably made the villages easier to defend and provided safe harbor for fishing boats, but residents had to carry water to their homes from wells on higher ground, often two or three miles away. Thus, Majeerteen coastal communities had little surplus labor even in good times.[8]

Before 1840, structural weaknesses in the Majeerteen economy did not work any great hardship on either herders or fishing peoples. Pastoralists who ran out of milk ordinarily sold more gum in exchange for rice. Coastal peoples who needed more labor lent their livestock and gum trees to pastoralists who had labor to spare and who benefited from the extra milk. During a drought, however, these measures alone did not always suffice to ensure a subsistence. Herders and fishing peoples then reduced their demand for luxury goods. During a drought in 1843, for example, herdsmen told Charles Cruttenden, a British official at Aden in charge of trade with the Somali country, that they rarely drank coffee. "If we drink

Society, London, vol. 177, pt. 2 (1966): 173–250, and "Vegetation Arcs in Somaliland," *Journal of Ecology*, 53 (1965): 57–67; H. B. Gilliland, "The Vegetation of Eastern British Somaliland," *Journal of Ecology*, 40 (1952): 91–124; J. E. G. W. Greenwood, "The Development of Vegetation Patterns in Somaliland Protectorate," *Geographical Journal*, 123 (1957): 465–73; W. A. MacFadyen, "Vegetation Patterns in the Semi-Desert Plains of British Somaliland," *Geographical Journal*, 116 (1950): 199–211; and S. B. Boaler and C. A. H. Hodge, "Observations on Vegetation Arcs in the Northern Region, Somali Republic," *Journal of Ecology*, 52 (1964): 511–44, and "Vegetation Stripes in Somaliland," *Journal of Ecology*, 50 (1962): 465–74. On wildlife found in the Majeerteen country in the nineteenth century, especially in the Nugaal Valley, see E. Blyth, "Report on a Zoological collection from the Somáli country," *Journal of the Asiatic Society of Bengal, Edited by the Secretaries*, 24 (1855): 291–306.

[7] Cruttenden, "Report on the Mijjertheyn," 114.

[8] Miles, "On the Neighbourhood of Bunder Marayah," 70–71; and Charles Graves to Chichi Graves, May 22, 1878, Graves Papers, p. 16.

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coffee once," they explained, "we shall want it again, and where are we to get it from?" Pastoralists also hoarded their livestock during droughts. In 1843, Cruttenden found that herders near the coast gladly sold their livestock to him, except in September, late in the hot season. He was forced to send men to higher pastures in the interior—a three-day walk—to buy meat on the hoof. Presumably, the Majeerteen also expanded their herds when the rains came in anticipation of periodic droughts as Somali pastoralists do today. In sum, the Majeerteen responded flexibly to unreliable rainfall with three strategies—mobility, interdependence, and a conservation ethic—all techniques that reflected a careful appraisal of their environment's limitations and of their own requirements for subsistence.[9]

How successful did these strategies prove when the rains failed? Between 1840 and 1843, the Majeerteen produced only one good crop of gum. In particular, 1843 proved, in Cruttenden's words, "very unfavorable, owing to the drought. The crop of gums [was] not more than half the average quantity." Cruttenden, however, saw few signs of destitution and made no mention of starvation. He reported seeing "fat" goats and sheep in "flocks [that] are immense" and "large droves of horned cattle," some weighing as much as three hundred pounds. He also reported that Majeerteen pastoralists hunted antelope and thereby "frequently provide[d] themselves with an abundant supply of fresh meat." The 1840–43 drought evidently did little to impair the ability of the Majeerteen to feed themselves. But in 1868 a drought produced famine. Why the difference? The answer lies in the growth of the Majeerteen sultanate and its leaders' efforts to expand regional production and trade, a transformation that undercut all three of the herders' subsistence strategies.[10]

THE FIRST MAJEERTEEN SULTAN, Maḥamuud (I), came to power sometime after 1809 and in a few years gained control over existing ports on the north coast. He appointed his sons to oversee trade there and to collect a portion of the loot taken from shipwrecks on those shores. Local chiefs (heads of mag-paying groups) who had dominated their own territory's trade and loot soon found themselves subordinate to the 'Ismaan lineage.[11] After Maḥamuud's death in 1818, his eldest son, 'Ismaan (I), became sultan and governed until about 1835. 'Ismaan entered into a partnership with a Somali merchant named Fatha Abdi, who lived in 'Alula,

[9] Cruttenden, "Report on the Mijjertheyn," 114. Cruttenden did not report that Majeerteen pastoralists increased their herds in anticipation of drought, but hoarding might have occurred on the interior plains. Hence, Cruttenden would have had no opportunity to observe that particular strategy, if it occurred.

[10] *Ibid.*, 114, 119, 122.

[11] Information on the sultanate is compiled from 'Ismaan genealogies and traditions. Cruttenden, "Report on the Mijjertheyn," 112–13, 116–17; Miles, "On the Neighbourhood of Bunder Marayah," 68–69; H. M. Abud, comp., *Genealogies of the Somál Including Those of the Äysa and Gadabürsi by Captain P. Z. Cox, Indian Staff Corps* (London, 1896), iv, 1, 3; Lewis, *Peoples of the Horn of Africa*, 100; Enrico Cerulli, "Tradizioni Storiche e Monumenti della Migiurtinia," *Africa Italiana*, 4 (1931): 153–60; John A. Hunt, *A General Survey of the Somaliland Protectorate, 1944–1950* (London, 1951), 146–49; Cassanelli, *Shaping of Somali Society*, 90–96; and I. M. Lewis, "Historical Aspects of Genealogies in Northern Somali Social Structure," *Journal of African History*, 3 (1962): 35–48.

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a north coast port that 'Ismaan ruled as head of the 'Ismaan Maḥamuud lineage, which populated the town. According to one European observer, 'Ismaan "considerably increased the trade [mainly in gum and frankincense] of the port." He protected his commerce by building the first stone forts in 'Alula. How Maḥamuud and 'Ismaan accomplished all this remains unclear, but, whatever the details, by 1835 the 'Ismaan lineage had consolidated its position as the most powerful group in the Majeerteen clan.[12]

'Ismaan's death led to a lengthy and violent crisis of succession out of which a larger and more complex regional political structure emerged. 'Ismaan's eldest son, Yuusuf, succeeded his father but reigned only two years, remembered in 'Ismaan traditions as "turbulent," including "quarrels" between Yuusuf and his 'Ismaan kinsmen.[13] But before Yuusuf could come to grief at his kinsmen's hands, he was murdered in 1837 by a member of the 'Ali Suleymaan lineage. The 'Ali Suleymaan hoped by the assassination to regain control over the loot from shipwrecks on the north coast and the external trade from their ports. After Yuusuf's death, they proclaimed their own sultanate and installed a certain Ismaa'iil Maḥamuud at its head. He established a capital at a small trading village, Candala, on the north coast, "so as to concentrate there the whole trade of the Majeerteen country," according to a later report.[14] Meanwhile, Yuusuf's death prolonged the succession crisis in the 'Ismaan lineage. Yuusuf had several sons, but none could rule under Majeerteen law and custom.[15] To break the dynastic impasse, Yuusuf's brother Nur-bin-'Ismaan conceived a solution that in one stroke produced a successor and the need for a regent to rule the country. At the time of Yuusuf's death, one of his wives was pregnant with her first child. Nur-bin-'Ismaan maneuvered to forestall the succession of another sultan until after the infant's birth. As it happened, the child was male, and Nur-bin-'Ismaan championed the infant, Maḥamuud (II), as the only legitimate successor to his father. Until Maḥamuud reached his majority, however, a council of Majeerteen chiefs ruled, and Nur-bin-'Ismaan, its most influential member, ran the sultanate's day-to-day affairs.[16]

Having resolved the 'Ismaan lineage's internal crisis, Nur-bin-'Ismaan "succeeded in checking the ambitious designs" of the 'Ali Suleymaan, but not for long. On August 1, 1842, the *Memnon*, a British steam frigate, wrecked on the Somali north coast near Cape Guardafui. The chiefs of three 'Ali Suleymaan and Siwakron villages dispatched their men to loot the ship. But they did not share the booty with the residents of nearby Bander Murayah where Sultan Maḥamuud and his uncle Nur-bin-'Ismaan resided. In response, Sultan Maḥamuud appealed for recompense to the Majeerteen Council, the body of clan elders who met yearly to

[12] Cruttenden, "Report on the Mijjertheyn," 116–17.

[13] *Ibid.*

[14] *Ibid.*; and Giulio Baldacci, "The Promontory of Cape Guardafui," *Journal of the African Society*, 9 (1909): 65.

[15] Cruttenden, "Report on the Mijjertheyn," 116. The ideal Majeerteen marriage was exogamous. Therefore, only a son issuing from a wife of Ḍulbahante, Warsangali, or Mareeḥaun origin could be considered a legitimate heir to the sultanate.

[16] *Ibid*; and Margaret Castagno, *Historical Dictionary of Somalia* (Metuchen, N.J., 1975), 25, 104–05.

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render binding judgments on offenses large and small committed by their clansmen. Because the 'Ismaan Maḥamuud constituted over half of the Majeerteen population, they invariably formed a majority in the council and "drown[ed] the clamors of the arrogant chiefs who reside[d] on the coast," according to a European observer. But before 1842 those chiefs had not included leaders of the 'Ali Suleymaan who had no direct kin relations with the Majeerteen clan. That fact notwithstanding, in January 1843 the council censured the 'Ali Suleymaan chiefs "for daring to appropriate to themselves property cast on the shore by the sea, without the consent of the 'sultan's house.'" In addition, the council levied a fine (of several valuable horses) on each offending chief, which the chiefs agreed to pay.[17]

By the early 1840s, then, Nur-bin-'Ismaan had consolidated his power over both the 'Ismaan Maḥamuud lineage and adjacent rival lineages and clans. He thereby secured exclusive control over shipwrecks and their loot and, hence, over the patronage that booty generated. But the sultanate had become more than Nur-bin-'Ismaan's personal tributary state consisting of the 'Ismaan lineage and a few client chiefs. Indeed, the Majeerteen clan itself became the sultanate, ruled by the leaders of the dominant faction in the Majeerteen Council. Their rule was ensured not only by the 'Ismaan majority in the council and control over booty but also by an 'Ismaan monopoly of mounted warriors. The Majeerteen clan no longer merely mediated local disputes but came to dominate all of the region's local political economies.

Over the next twenty years, the Majeerteen's new regional political structure enabled Nur-bin-'Ismaan to expand domestic production and trade and to challenge Indian merchants for control of overseas commerce. Specifically, Nur-bin-'Ismaan sought to take advantage of increased European traffic in the Gulf of Aden. Between 1800 and 1840, British, French, and American traders began to compete with Indian and Arab merchants for dominance of the Indian Ocean trade. To protect British merchant vessels and mail packets plying the Gulf, the East India Company initiated patrols from Bombay to Suez with India Navy steam frigates. About 1839, the Company established a coaling station at Aden to serve that shipping. British actions had two major consequences for the region around the Horn. First, they vastly increased the number of ships sailing in the Gulf of Aden and, hence, shipwrecks, booty, and clients for the Majeerteen sultanate. Second, British commercial activity created a demand for foodstuffs at Aden to supply a local garrison and passing ships. Berbera soon became the major entrepôt for livestock raised in the western Somali hinterlands. The Majeerteen acted as supplementary suppliers to the British.[18]

[17] Cruttenden, "Report on the Mijjertheyn," 117–18.

[18] R. J. Gavin, *Aden under British Rule, 1839–1967* (New York, 1975), 22–28; Harvey Sicherman, *Aden and British Strategy, 1839–1968*, Research Monograph Series, no. 12 (Philadelphia, 1972), 7–14; Frederick Cooper, *Plantation Slavery on the East Coast of Africa* (New Haven, Conn., 1977), 38–46; Richard Pankhurst, "The Trade of the Gulf of Aden Ports of Africa in the Nineteenth and Early Twentieth Centuries," *Journal of Ethiopian Studies*, 3 (1965): 36–81, and "Indian Trade with Ethiopia, the Gulf of Aden, and the Horn of Africa in the Nineteenth and Early Twentieth Centuries," *Cahiers d'Etude Africaines*, 10 (1974): 453–97; and M. Abir, "Caravan Trade and History in the Northern Parts

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Sometime after 1839, Nur-bin-'Ismaan, using his new dominance of the Somali coast as a bargaining chip, concluded an agreement with the British at Aden. The regent evidently hoped to attract British buyers in order to break the monopoly of Indian merchants over the north coast trade. According to Cruttenden, "The people are perfectly aware how much they are pillaged, and earnestly hope that some of the ships that they so frequently see passing along their coast might be induced to come in and trade with them." In the agreement, Nur-bin-'Ismaan promised to extend the services of an *abbaans* to all British shipwreck survivors on the Majeerteen shore. For this the British agreed to pay the sultan an annual annuity of three hundred sixty Maria Theresa dollars. British officials at Aden also apparently agreed to purchase livestock from Majeerteen merchants. In 1843, British officials from Aden began to supervise the recovery of survivors from British ships wrecked on the north coast, and Majeerteen merchants began shipping livestock to Aden. Livestock exports rose from none at all in 1839 to fifteen thousand animals in 1844. Indeed, demand increased for a variety of goods produced by Majeerteen pastoralists. In 1842, one European traveler noted that "the advantage of Aden as a mart is . . . felt in the increasing activity throughout the northern [Somali] districts to produce for that market the staple commodities of the land, hides, gum, cattle, sheep, &c."[19]

Trade in livestock had two important consequences for the Majeerteen. First, it forced herders to increase substantially the size of their herds in order to produce fifteen thousand beasts a year above their own requirements for subsistence. As a result, Majeerteen pastoralists dispersed their herds over a larger area and into new lands. The Nugaal Valley, occupied by the 'Iise Maḥamuud, experienced the greatest influx, mainly from Majeerteen goat herds. Further south, the 'Umar Maḥamuud began to encroach on the fertile plains belonging to the Mareeḥaun clan.[20] Second, the demand for livestock produced a class of wealthy Majeerteen traders and rentiers. By 1843, 'Ismaan political leaders operated twelve vessels—all but one of which sailed exclusively to Aden and back—out of Majeerteen ports on the north coast. From that commerce, Majeerteen merchants accumulated capital and invested it in livestock. In 1843, Cruttenden reported that "some of the principal Bedouin Chiefs" possessed "upwards of a thousand she camels" and even more goats and sheep. The chiefs parceled out their herds of fifty to eighty animals to their wives and clansmen, who became clients as well as kin. In addition, Majeerteen traders bought usufruct rights to the country's marble hills where acacia trees grew wiid. Earlier, herders had claimed acacia plots held in common by the sultan for their own use at no cost. After 1843, the 'Ismaan sultanate sold

of East Africa," *Paideuma*, 14 (1968): 115. For evidence of an expansion of trade after 1800 among Arab and Indian merchants on the Somali coast, see Cooper, *Plantation Slavery on the East Coast of Africa*; and Pankhurst, "Trade of the Gulf of Aden Ports." On the impact of the expansion of regional markets on trade with interior peoples, see Abir, "Caravan Trade and History."

[19] Cruttenden, "Report on the Mijjertheyn," 113, 120, 122; and W. Christopher, "Extract from a Journal by Lieut. W. Christopher, Commanding the H.C. Brig of War 'Tigris,' on the E. Coast of Africa," *Journal of the Royal Geographical Society of London*, 14 (1844): 101.

[20] Cruttenden, "Report on the Mijjertheyn," 120.

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those rights to merchants, who then rented acacia plots to herders in exchange for a portion of the gum crop. Pastoralists became tenants, caring for Majeerteen merchants' livestock and gum trees, although they were somewhat better fed for their trouble than before.[21]

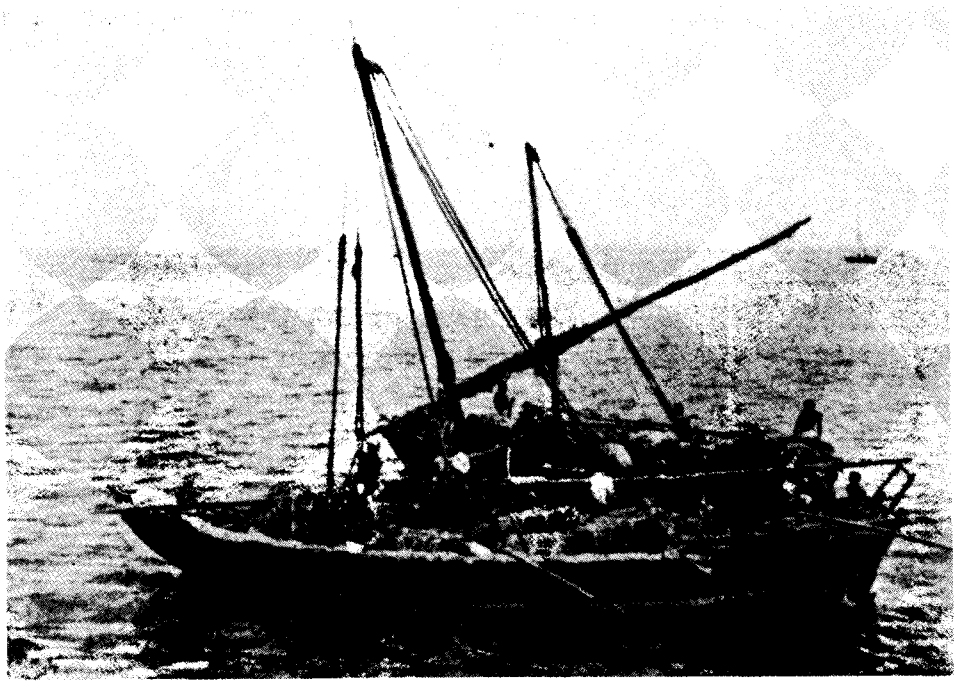
Having transformed Majeerteen production, Nur-bin-'Ismaan set out to turn the region's trade to the sultanate's profit. But a group of fifteen Indian and Arab merchants, mostly Banyans from Bombay, stood in his way. Each year they came to trade on the Majeerteen north coast in October and November at the close of the monsoon season in the northeast. At Bander Khor in 1843, Cruttenden observed 712 tons of gum being loaded on small ships of forty- to fifty-ton capacity. Over half of that gum went to Bombay, a third to the Red Sea ports, and a sixth to the south Arabian coast. To get the gum, Indian and Arab merchants welcomed herders from the interior who, because of the hot season, had trekked to the coast without food, hoping to sell gum in exchange for cash or rice. When the pastoralists' gum supplies ran out, Banyan traders then sold foodstuffs to the herders on credit. According to Cruttenden, "A running account is carried on from year to year, which of course the wary creditor takes care never to settle." Only two Majeerteen merchants had a hand in this trade. Indian and Arab merchants also supplied imported goods to the Majeerteen. In addition to rice, they sold dates from Arabia, tobacco from Surat, dungaree cloth, coarse white American sheeting, blue-striped turbans, and small bars of iron. Profits on these goods were very high. Dates, for example, could be sold to the Majeerteen for twelve times their cost in Muscat. Indian and Arab merchants extended this trade to the south coast during the southwest monsoon season.[22]

In the 1840s, Nur-bin-'Ismaan conceded Indian dominance of trade in the Majeerteen north coast ports but sought to open an entirely new port to attract livestock and gum from the Warsangali country to the west and the 'Iise Maḥamuud country to the south. Before the drought in 1842, most trade from those areas passed through the small port of Bander Zeyada. It had been founded about 1800 by "nomads [probably the Wabenaya] from the interior, who gradually came to live on the sea-shore, attracted by the prospect of trade." That trade came chiefly from a crossroads community called Karin, located about a six-hour walk inland, where the Daghan, a perennial stream with a "strong current during the rainy season," passed through a mountain gorge. Good water and "abundant" vegetation made it a rest stop for caravans traveling to the coast from as far away as the Nugaal Valley. Around 1840, Nur-bin-'Ismaan attempted to siphon off some of that trade. He established a port, El Bet Nur (house of Nur), at the mouth of the Daghan, about a three-hour walk to the east of Bander Zeyada. The port,

[21] *Ibid.*; and Charles Guillain, *Documents sur l'histoire, la géographie et le commerce de l'Afrique orientale*, 3 vols. (Paris, 1856), vol. 1, pt. 2: 436–37.

[22] Cruttenden, "Report on the Mijjertheyn," 122, and "Memoir on the Western or Edoor Tribes, inhabiting the Somali Coast of N.-E. Africa, with the Southern branches of the family of Darrood, resident on the banks of the Webbe Shebeyli, commonly called the River Webbe," *Journal of the Royal Geographic Society*, 19 (1849): 63.

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"Dhows Laden with Hides and Skins," Ralph E. Drake-Brockman, *British Somaliland* (London, 1912), facing page 244. Photograph reproduced courtesy of the Library of Congress, Washington, D.C.

however, fared poorly, perhaps as a result of raiding induced by the drought in 1842.[23]

When the drought ended, Nur-bin-'Ismaan hoped once again to tap the interior caravan trade passing through Karin. Sometime in the early 1840s, the Kaptallah, a small seafaring group of Somalis, had established the port of Bander Gassim, which was a walk of about three and one-half hours east from El Bet Nur. The prospect of trade, however, soon attracted two more groups, the Dechichi, a local herding lineage only distantly related to neighboring peoples, and the "Bagaren" (probably the Abokor lineage of the Ḍulbahante), who "came down from the interior, envious of the increasing trade and prosperity of the coast." Each group evidently hoped to bypass existing coastal traders and to sell their livestock directly to buyers in Aden. But each group also proposed to act as a middleman for all others in this new commerce. Therefore, the Bagaren attacked and defeated the Dechichi with guns supplied by Arabian allies and constructed a fort to defend their new monopoly. The Dechichi in turn appealed to Nur-bin-'Ismaan for help. He sent some of his 'Ismaan kinsmen to settle there, hoping to blunt Bagaren control of the port's trade. Within a year that effort emboldened the Dechichi to challenge the Bagaren to open combat. But they lost again because the Bagaren controlled the town's only fort. Nur-bin-'Ismaan and his Dechichi allies then called for a peace conference. They invited elders from each clan and lineage at the port, among them, Nur Maḥamuud, the Bagaren chief. At that meeting, Nur Maḥamuud was assassinated, and Bagaren dominance of the port ended. Thus,

[23] Baldacci, "Promontory of Cape Guardafui," 59–61.

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with a single treacherous act, the 'Ismaan sultanate acquired the Dechichi as clients and gained control over a vast and productive hinterland.[24]

Bander Gassim proved crucial to the growth of the sultanate's economy. In the 1850s it provided Majeerteen merchants access to gum free from competition by Indian and Arab merchants. As a result, 'Ismaan caravans from Bander Gassim for the first time penetrated the interior. These merchants acted as retailers and carried with them goods purchased from Arab and Indian traders. They took care to arrive before herders made their way to the port towns in September, knowing that pastoralists would sell their gum and livestock on the spot to avoid the long journey to the coast. In doing so, herders gave up their right to bargain with competing merchants for higher prices. Majeerteen and Warsangali pastoralists therefore sold their products cheaply and purchased goods at prices that reflected both the caravan trader's cut and the Indian merchants' profits.

Expansion of the Majeerteen internal market in turn stimulated 'Ismaan merchants to trade overseas. They quickly increased their share of the gum trade earlier dominated by Indian and Arab traders. According to one rough estimate, Majeerteen gum exports rose from seven hundred fifty tons in 1843 to between one thousand and fifteen hundred tons in 1856. In 1854, Charles Guillain, a French naval officer sent to survey the east coast of Africa, called Bander Gassim "the Majeerteens' most important village." Ships from Muscat, Bombay, and Aden docked at the port to take on livestock, gum, incense, and myrrh. Thereafter, the sultan, young Maḥamuud, made it his occasional residence and built three more stone forts in the town.[25] In the same decade, 'Ismaan merchants for the first time traded on the East African coast, another area where Arab merchants had been dominant. The 'Ismaan shipped dried fish from the Majeerteen east coast to several points south, including the ports of M'Kellé on the modern Kenyan coast and Zanzibar. There they exchanged fish for African millet, which in the 1850s replaced Arabian rice in the diets of Majeerteen herders. On their return voyage to the Horn, 'Ismaan traders exchanged millet for gum brought by the herders. Finally, the sultanate extended its trade to the southern Somali coast. About 1868 an armed Majeerteen force invaded and captured the port of Kismaayo at the mouth of the Juba River, the center of a rapidly developing agricultural region. At Kismaayo, through 1900, Majeerteen merchants purchased cattle and slaves from interior peoples for resale in Arabia.[26]

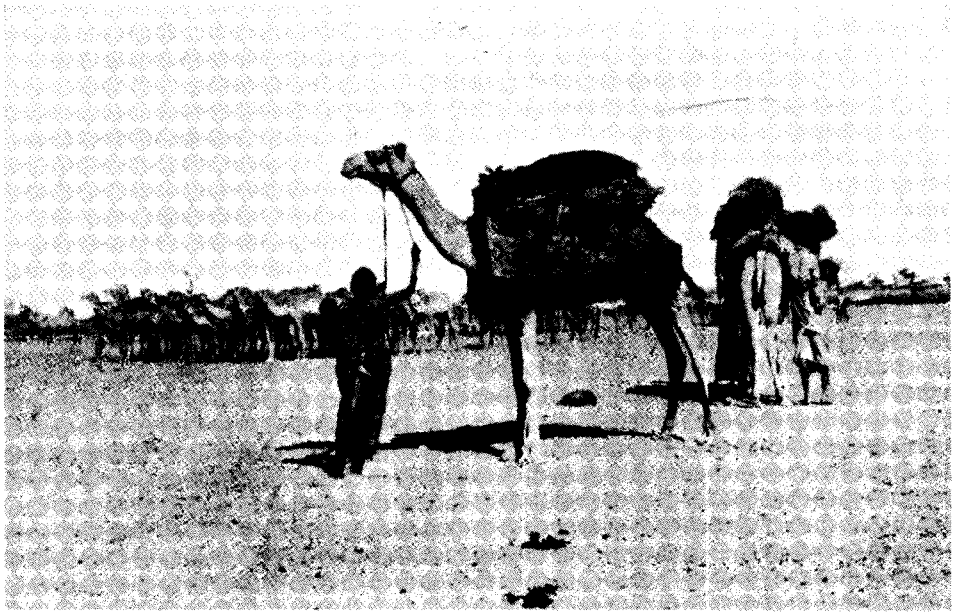
'Ismaan leaders wielded their political power to promote greater production and export. Herders and traders now roamed widely in remote territories claimed by neighboring clans, which evidently led to disputes over the control of pastures and numerous attacks on the Majeerteen. Therefore, Sultan Maḥamuud (II) periodically organized military expeditions and fought small skirmishes to defend his kinsmen's interests. Moreover, the sultan mobilized his 'Ismaan warriors to ensure

[24] *Ibid.*; and Guillain, *Documents sur l'histoire*, 407. Bander Gassim is shown on modern maps as Boossaaso.

[25] Guillain, *Documents sur l'histoire*, 390, 395–97, 408–10, 425–26, 444, 454–59.

[26] *Ibid.*, 458–59; and Marguerite Ylvisaker, *Lamu in the Nineteenth Century: Land, Trade, and Politics*, African Research Studies, no. 13 (Boston, 1979), 120–21.

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"A Caravan *En Route* to the Coast," Ralph E. Drake-Brockman, *British Somaliland* (London, 1912), facing page 112. Photograph reproduced courtesy of the Library of Congress, Washington, D.C.

domination of the clan's territories and peoples. In the mid-1850s, Guillain found Sultan Maḥamuud deep in the interior busy "pacifying a tribe in revolt." To pay for these expeditions, Maḥamuud became a roving tax collector, beginning in 1855 when he turned eighteen and assumed the title of sultan. Earlier sultans had resided on the coast and spent their time trading. Maḥamuud, however, chose to live inland among his herding kinsmen, moving from place to place, while Nur-bin-'Ismaan cared for the sultan's commercial interests in the country's ports. Maḥamuud visited the coastal towns only twice a year and then only long enough to collect his tribute from local chiefs who in turn taxed merchants.[27]

Sultan Maḥamuud also collected taxes as part of a coherent fiscal policy designed to promote economic development. First, he imposed several direct levies on production. The largest was a 20 percent tax in kind on gum and frankincense. Landlords and tenants each paid one-half of the tax on crops grown for shares. The sultan also demanded one-sixth of all the camels in each Majeerteen herd and one-tenth of all horses but did not tax other livestock, most notably goats raised for export to Aden. Maḥamuud thereby promoted the livestock trade dominated by Majeerteen merchants at the expense of Indian and Arab gum traders. Second, Maḥamuud levied an indirect but fixed tax on descent groups. He required each lineage chief to remit one-third of his receipts, presumably part of the contribution individuals made yearly to their sublineage.[28] This tax protected the sultanate's capital reserves. Finally, on the coast, Maḥammud collected export and import duties from merchants. He placed a levy of one-quarter of a Maria Theresa dollar

[27] Guillain, *Documents sur l'histoire*, 397, 411, 438, 440; and Charles Graves to Chichi Graves, May 22, 1878, Graves Papers, pp. 20–21.
[28] Guillain, *Documents sur l'histoire*, 443–44, 457.

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on each *bohar* (about one-tenth of a ton) of gum, incense, and myrrh exported from Majeerteen ports. That amounted to a little less than 1 percent of each item's local value. The sultan taxed imports more heavily. He charged 5 percent *ad valorum* and thereby maintained a favorable balance of payments by encouraging exports over imports.[29]

But the 'Ismaan rulers' success in promoting large-scale production and trade soon led to periodic economic disaster as a result of rapid ecological deterioration. First, by expanding their herds onto marginal lands, the Majeerteen exchanged selective grazing for an intensive use of all pastures. They thereby gave up the mobility essential to their exploitation of a variable and delicate landscape. Second, a new regional debt structure created by a handful of merchants destroyed the interdependence of herders and fishing peoples. Majeerteen producers therefore had little choice but to sell all of their gum at low prices in exchange for food, which put them in debt to coastal merchants. Simpler ties between a few traders and a mass of debtors replaced an older and more complex set of relations between pastoralists and coastal residents. Finally, the 'Ismaan trading monopoly and its fiscal policy enabled merchants to require expanded production from their clients. Pastoralists had no choice but to raise and sell livestock in order to satisfy creditors and pay taxes. Thus, herds continued to grow even during droughts, killing pastures as they grazed. Pastoralists' efforts to curb their consumption no longer resulted in reduced production. Hence, Majeerteen herders became infinitely more vulnerable to any variability in their natural resources, especially water. Between 1866 and 1880, that vulnerability killed hundreds of the Majeerteen during a lengthy drought.[30]

In 1868, widespread drought produced famine for the first time in the Majeerteen country. Residents there told S. B. Miles, a British officer from the Aden residency, that "a large portion of the inhabitants . . . died of starvation." In 1872, Miles recorded what he saw in the Dharor Valley. Beginning at the coastal village of Hunda, he entered the valley and moved up the river trench. Only by digging two or three feet into the stream bed could water be found. Wild palm trees grew there but little else. Villagers wove palm leaf mats for export and appeared "miserably poor." "There is no cultivation, and the fruit of the wild date seems a staple article of food." Later, about sixty miles farther upcountry, he observed,

[29] *Ibid.*, 444; and Charles Graves to Chichi Graves, May 22, 1878, Graves Papers, pp. 20–21.

[30] For studies on famine as a political event, see Paul Richards, "Ecological Change and the Politics of African Land Use," *African Studies Review*, 26 (1983): 41–50; and Sara S. Berry, "The Food Crisis and Agrarian Change in Africa: A Review Essay," *African Studies Review*, 27 (1984): 59–112. On herding and the ecology of Somalia, see Jeremy Swift, "Pastoral Development in Somalia: Herding Cooperatives as a Strategy against Desertification and Famine," in Michael H. Glantz, ed., *Desertification: Environmental Degradation in and around Arid Lands* (Boulder, Colo., 1977), 275–305, and "The Development of Livestock Trading in a Nomad Pastoral Economy: The Somali Case," in L'equipe écologie et anthropologie des sociétés pastorales, *Pastoral Production and Society/Production Pastorale et Société* (Cambridge, 1979), 447–65; and Z. A. Konczaki, "Nomadism and Economic Development of Somalia: The Position of the Nomads in the Economy of Somalia," *Canadian Journal of African Studies*, 1 (1967): 165. Swift examined the subsistence strategies of Somali pastoralists and concluded that the effects (including famine) on the Somali economy of demands for livestock from external markets occurred in the twentieth century. The case of the Majeerteen points to changes occurring much earlier.

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"The universal drought in the country had visited this place: they had hardly had any rain for a year, and the river was dry in consequence. All human habitations had now ceased, and I was told that none were to be met with for a long distance in front." Along the way, he could find no gum trees in the hills, and he saw very little wild game. In the same drought, "thousands of animals—camels, sheep, and horses"—died as well. Majeerteen informants pointed out to Miles one man who had lost 384 of his 400 camels. "Many were the stories of large proprietors who had been utterly ruined." By 1872, interior herdsmen were refusing to part with their animals and would not even sell *ghee*, which was usually exported to Arabia, to the coastal Majeerteen.[31]

Majeerteen coastal peoples fared somewhat better. Fish, of course, remained available. Because the drought killed many gum trees, coastal villagers began to plant saplings on their hills, rather than simply exploit the trees that grew wild there. They also tried other kinds of plant husbandry. Between 1869 and 1872, on the plain between Bander Marayah and Cape Felix, villagers planted date palms and millet near their wells "as an experiment." Majeerteen villagers in the two valleys south of Cape Guardafui began small-scale irrigation farming. They learned from an Arab there methods of cultivating maize, tobacco, millet, dates, palms, tamarinds, plantains, and onions. Coastal dwellers reallocated labor rendered surplus by the drought into new channels, mainly to produce alternative goods for internal consumption. Instead of tending pastoralists' flocks destined for sale overseas, women and children hauled water to grow their own foodstuffs. Majeerteen clansmen who dwelt near the ocean appeared to Miles "ready and willing enough to turn to agriculture."[32] The coastal Majeerteen fared better than the herders because they did not purchase what they needed for subsistence on a regional market. Production decisions on the coast remained in the hands of local heads of household who respected the limits of their resources. The drought inconvenienced the coastal Majeerteen; it did not kill them.

But herders experienced the drought's ill effects many times over. First, competition for access to pastures and wells led to blood feuds and skirmishes between herding sublineages. Second, disorder engendered by the drought attracted a new enemy to the sultanate—the Khedive Ismaa'iil of Egypt who hoped to make it part of a larger East African empire. Finally, the sultanate's rivals, especially the 'Ali Suleymaan, took the region's crisis as an opportunity to settle old scores and regain lost prerogatives. When 'Ismaan (II), the son of Maḥamuud (II), succeeded his father on Maḥamuud's death in 1866, the troubles began.[33]

In 1868, the first year of famine, conflict broke out between the Majeerteen and the Warsangali, fellow members of the Harti clan confederation who lived just west

[31] Miles, "On the Neighbourhood of Bunder Marayah," 63, 74–75. The drought and famine that began in 1868 were not confined to the Majeerteen country but included the Aden area, which no doubt increased pressure on Majeerteen herders to sell their remaining livestock. Hunter, *Account of the British Settlement*, 178.

[32] Miles, "On the Neighbourhood of Bunder Marayah," 71.

[33] *Ibid.*, 69.

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of the Majeerteen. According to an 'Ismaan tradition, the dispute resulted from the killing of a Majeerteen man by a Warsangali clansman. Most likely, the Warsangali and Majeerteen had plundered each other's rapidly disappearing herds. The killing simply provided an excuse to carry out raiding on a larger scale. But such conflicts in the past had involved only a few dozen men engaged in isolated attacks. In 1868 the fighting quickly escalated to a level never before reported by 'Ismaan traditions or European observers. Local skirmishes among individuals and sublineages gave way to general warfare between a tributary state and its neighboring clans. The Majeerteen sultan raised an army of seven thousand men, nearly every able-bodied adult male in a population of perhaps twenty-five thousand. The smaller Warsangali clan mustered only four thousand. Not surprisingly, the sultan's larger army prevailed, aided by new firearms unavailable to its opposition. The Majeerteen killed eight hundred Warsangali men in a nine-hour battle. Later, the Ḍulbahante, a third Harti clan that had allied with the Warsangali, lost about six hundred men in a separate confrontation with the 'Ismaan Maḥamuud. Charles Graves, an officer in the Egyptian army, reported that the Ḍulbahante army was "nearly exterminated."[34]

Meanwhile, agents representing Khedive Ismaa'iil attempted to bring the Horn under Egyptian rule. In 1876, Egyptian military officers approached the chief of Hunda and offered him a protectorate. He accepted, but the protectorate had little value.[35] In 1877, however, the British government made a secret treaty with Egypt that assigned to the khedive several territories nominally belonging to the sultan of Zanzibar, including all of the Horn. 'Ismaan ignored the agreement. But, in May 1878, assured of British acquiescence, Khedive Ismaa'iil dispatched an armed expedition to survey the Majeerteen coast. He hoped to occupy the sultanate and transform it into "military colonies." But financial difficulties at home and the Mahdist revolt forced the khedive to abandon his plan to seize the Somali coast.[36]

Yuusuf 'Ali, a wealthy merchant, chief of 'Alula, and head of the 'Ali Suleymaan, gave 'Ismaan more trouble. In 1878 the 'Ali Suleymaan refused to build a residence in 'Alula for Sultan 'Ismaan, claiming that they had built one for the sultan's father and therefore had done their duty. They further asserted that the real difficulty between the groups lay in the sultan's efforts to depose Yuusuf 'Ali and rule 'Alula directly. In their view, Yuusuf 'Ali simply had grown too rich and influential for the 'Ismaan sultanate to tolerate. The conflict worsened when Yuusuf 'Ali and his followers plundered two wrecked steamers at Cape Guardafui without reserving for the sultan his customary share. Between twenty-five hundred and three

[34] Gavin, *Aden under British Rule*, 147–49; Charles Graves to Chichi Graves, May 22, 1878, Graves Papers, pp. 21–22; Crabitès, *Ismail: The Maligned Khedive*, 138–41, and *Americans in the Egyptian Army*, 228–38; E. R. Turton, "Kirk and the Egyptian Invasion of East Africa in 1875: A Reassessment," *Journal of African History*, 11 (1970): 355–70; and Hamilton, "Imperialism Ancient and Modern," 19–33.

[35] Charles Graves to Chichi Graves, May 22, 1878, Graves Papers, p. 9.

[36] *Ibid.*, pp. 28–31; E. A. Stanton, ed., "Secret Letters from the Khedive Ismail in Connection with an Occupation of the East Coast of Africa," *Journal of the Royal African Society*, 34 (1935): 269–86; and *Military Report on Somaliland*, 121–22.

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thousand clansmen of the 'Ali Suleymaan converged on the beach, "coming from a distance of 75 to 80 miles in all directions." Yuusuf 'Ali then sent the scavenged goods directly to Aden for sale. He reportedly gained a fabulous sum for his efforts and divided the proceeds among his people. Thus, Yuusuf 'Ali succeeded in breaking the 'Ismaan monopoly over booty and patronage.[37]

In 1880 the failure of the rains produced the second famine in twelve years among herders. According to one report, Majeerteen clansmen along the coast sent their remaining livestock to the interior in search of pasture and water. Therefore, they had no milk to sustain themselves. Those who could do so hunted for wild game, chiefly gazelle and antelope, but with little to show for their efforts. At the same time, their supply of gum was exhausted, and they could not bargain for millet or rice. Import-export trade ceased in the port towns. The result was malnutrition, sickness, and "atrocious misery," in the words of George Revoil, an explorer for the French government who spent nearly a year in the Majeerteen territory. The drought produced worse conditions inland. In the Dharor Valley, the 'Iise Maḥamuud were reduced to eating grass and seeds from trees. Revoil visited one encampment where a dozen emaciated women huddled around a fire cooking a pot of leaves for their meal. Their children nearby chewed "ravenously" at a branch cut from a bush. Women and children begged the Frenchman for food. Other 'Iise Maḥamuud people trekked to the coast to exchange their last possessions for whatever food they could find.[38]

Herders began to raid one another for the little food that was left. The 'Iise Maḥamuud pillaged the 'Ismaan Maḥamuud. The 'Ismaan Maḥamuud in turn robbed Ḍulbahante herders. While Revoil remained in Bander Gassim, in fact, the chief source of livestock and milk in the market there came from camels and goats captured from the Ḍulbahante. In December 1880, war broke out. Ḍulbahante warriors invaded the Dharor Valley and threatened to attack Bander Gassim. Residents there reportedly went about their business armed and on "continual alert." Whether that attack came we do not know, but the Ḍulbahante were in retreat by January 1881. The Warsangali had attacked them from the north. The 'Iise Maḥamuud to the south, meanwhile, had regained control of the Dharor Valley. To Revoil, the Majeerteen country appeared "constantly at war."[39]

These battles involved single lineages and villages and were smaller and less organized than those of 1868, probably because the struggle between the 'Ismaan and 'Ali Suleymaan lineages had become open warfare by the end of 1880. By December, 'Ali Suleymaan leaders had accepted protectorate status and perhaps military aid from the Italian government. In addition, Yuusuf 'Ali claimed twenty-five hundred to three thousand new client-warriors who had shared in the booty from the two shipwrecks that his clan looted. 'Ali Suleymaan leaders, therefore, were able to attack 'Ismaan from their base at 'Alula. In retaliation, the

[37] Charles Graves to Chichi Graves, May 22, 1878, Graves Papers, p. 20.
[38] Revoil, *La Vallée du Darror* (Paris, 1882), 94, 102–03, 126, 131, 133, 135, 139, 151, 351.
[39] *Ibid.*, 20, 95–102, 185.

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sultan attacked 'Alula and succeeded in taking it on January 15, 1881. But Yuusuf 'Ali and his army escaped. They retreated into the adjacent Siwakron territory where Yuusuf 'Ali convinced the Siwakron to join him against 'Ismaan. Worse still for the sultan, Yuusuf 'Ali persuaded the large 'Iise Maḥamuud lineage to participate in a general attack to surround and destroy 'Ismaan and his army.[40] The 'Iise Maḥamuud dominated the Nugaal Valley, and their chief, nominally subordinate to the sultan, controlled his own force of fifteen hundred horsemen.

Whatever the exact outcome of these battles, the 'Ismaan Maḥamuud and 'Ali Suleymaan lineages reached a military stalemate by 1883 and finally compromised. 'Ismaan confirmed Yuusuf 'Ali's position as 'Alula's legitimate chief. In return, Yuusuf 'Ali accepted the sultanate's paramountcy, presumably agreeing to abide by an 'Ismaan monopoly of booty. To ensure the deal, the sultan married Yuusuf's daughter, Aisha. Each would reign supreme in his own sphere, but Yuusuf 'Ali temporarily disclaimed greater ambitions because 'Ismaan held his daughter as a pawn. This solution did not last long. In 1883, with Italian support, Yuusuf 'Ali organized a sultanate of his own at Hobyo on the south coast below the Majeerteen border, which left many in the 'Ali Suleymaan lineage stranded at 'Alula and especially vulnerable when 'Ismaan moved his permanent residence there. But, in 1884, 'Ismaan and Yuusuf worked out another agreement that permitted 'Ali Suleymaan people to live and trade in peace at 'Alula and to retain control of the wells there.[41] Separate sultanates resolved the internal political turmoil that had plagued the Majeerteen since the early nineteenth century.[42]

Unfortunately, the 1884 settlement between 'Ismaan and Yuusuf 'Ali did little to solve Majeerteen economic problems. Production for export continued to erode the region's limited natural resources. Moreover, after 1900, trade in gum and livestock shifted away from Majeerteen ports to Berbera, where British officials enlarged the harbor and provided greater security for caravans traveling from the Dharor and Nugaal valleys. The decline of the Majeerteen trade caused several port towns to disappear. Berbera also attracted new Arab and Indian merchants who broke the Majeerteen's monopoly of the livestock trade. But the decline of the Majeerteen sultanate did not signal a return to a small-scale subsistence economy.

[40] *Ibid.*, 29, 77, 156–57.

[41] *Military Report on Somaliland*, 124–25; Robert L. Hess, *Italian Colonialism in Somaliland* (Chicago, 1966), 8, 127; "Luigi Bricchetti Robecchi's Journeys in the Somali Country," *Geographic Journal*, 2 (1893): 361; and "Editorial Notes," *Journal of the African Society*, 8 (1909): 429.

[42] Lewis, *Modern History of Somaliland*, 99; Baldacci, "Promontory of Cape Guardafui," 59; and *Military Report on Somaliland*, 122–23. In the 1880s and 1890s, Sultan 'Ismaan regrouped his lineages into another single unified sultanate. He ruled without challenge until the 1920s, when Italy attempted to make good on its claims to the Majeerteen country made in the Convention of 1899. In 1925 the Italian army attacked. Sultan 'Ismaan, now an old man, rallied his forces and held off the Italians, twelve thousand soldiers in all, for two years. Finally, an Italian naval blockade cost the sultan his position by cutting off the Majeerteen's overseas trade, the sultan's chief source of income and his peoples' last resort for food during the dry season. In 1927 Sultan 'Ismaan received a pension and was sent to Mogadishu, where he lived until his death in 1943.

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Herders continued to expand their herds to provide livestock for export, which meant periodic depletion of pastures. As a result, droughts led to famines every few decades throughout the early twentieth century.[43]

BY THE MID-NINETEENTH CENTURY, production for export had undercut herders' subsistence strategies by reducing the control pastoralists had over their kinsmen's labor. Decisions once made locally and once responsive to a delicately balanced environment fell into the hands of livestock buyers in Aden who had little stake in the condition of Somali pastures. Herders therefore had to abandon their interdependence upon coastal peoples, their mobility over a terrain poor in resources, and their practice of conservation during recurring droughts. As a result, they became more vulnerable to changes in the supply of water, their most important resource. Natural cycles of drought and rain had once brought modest prosperity and occasional deprivation. After mid-century, variable rainfall engendered immense but temporary wealth among herders followed by starvation and war. Only the Majeerteen coastal peoples avoided participation in capitalist markets and thereby retained control over their own labor and lands. They diversified their local economy and prospered by subsistence agriculture.

The origins of famine in northern Somalia lay not in active imperial exploitation but in the sultanate's hasty commitment to capitalism. For a time, Majeerteen leaders created an uneasy partnership of two political and economic systems, one aiming toward subsistence and another pursuing profit, one organized around kinship and another structured by ties between patrons and their clients. This was a political economy at war with itself. Herders required flexibility in managing the size of their herds, but British buyers and Majeerteen merchants demanded continuous production and trade regardless of the ecological consequences. As a result, the pursuit of profits by 'Ismaan leaders undermined their clients' ability first to subsist and then to allocate by peaceful means political power among themselves. In doing so, the sultanate rendered Majeerteen clansmen and their families vulnerable to famine and made them easy prey to internal dissension and external enemies—an enduring legacy, indeed, of "atrocious misery."

[43] *Military Report on Somaliland*, 62, 86; Baldacci, "Promontory of Cape Guardafui," 72; and I. M. Lewis, "Lineage Continuity and Modern Commerce in Northern Somaliland," in Paul Bohannan and George Dalton, eds., *Markets in Africa* (Evanston, Ill., 1962), 365–85.

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Anglo-Indian Medical Theory and the Origins of Segregation in West Africa

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In 1880 a British army officer in India argued strongly in favor of measures "which alone can secure that segregation of the mind required for our own political welfare, but which cannot possibly be maintained by a mere segregation of the bodies."[1] His use of the word "segregation" appears to be strikingly like the current definition in *Webster's Third New International Dictionary*—"the separation or isolation of a race, class, or ethnic group by enforced or voluntary residence in a restricted area, barriers to social intercourse, divided educational facilities, or other discriminatory means"—that is, the conscious manipulation of physical space on the part of a dominant group in order to achieve or maintain a psychological gap between itself and a group it intends to keep in an inferior place. Actually, the officer was defending the autonomy of the Punjab (Irregular) Field Force, which he happened to command and which was then under threat, in the interest of improved military efficiency, of being consolidated into a unified Indian army under a single commander-in-chief. Harking back to the experience of the great Mutiny of 1857–58, the officer was discussing how best to keep apart Punjabis and Hindustanis, the so-called martial races of northern India, thus preventing them from combining in a race war against the white man. For him, segregation was a means to promote the classic strategy of divide and rule, not a way to regulate relations between black (or rather brown) and white.

Segregation was not a key word in the vocabulary of race relations among English-speaking peoples in the nineteenth century. The definition found in the third edition of Webster's dictionary is not included in the *S* volume of the *Oxford English Dictionary* (*OED*), which was compiled between 1908 and 1914. (It is, of course, in the more recent supplement.) Most of the definitions in the earlier edition of the *OED* refer to religion, the natural sciences, and medicine: the segregation of Calvinist from Catholic congregations, of elements in a chemical compound, of sick persons considered contagious (meaning that their bodies contained a poison, seed, or "germ" that could be passed directly to others).

The history of words is primarily a clue to theory or consciousness, from which behavior must be distinguished. The practice of placing different races, classes, and ethnic groups in separate areas, either to restrict contact thought undesirable

[1] Minute of General E. B. Johnson, May 4, 1880, Great Britain, *Parliamentary Papers* (hereafter, *PP*), 1884–85 (17), vol. 59, Papers re Indian Army Reorganization.

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and perhaps potentially dangerous or to accommodate a group that prefers to live that way, is of course old and by no means limited to the English-speaking world. The medieval Jewish ghetto, separate areas for barbarian (that is, non-Chinese) merchants in China, distinct quarters of believers and infidels in the cities of North Africa and the Middle East are examples that come to mind. For centuries Indian castes had practiced segregation, regulated by complex and pervasive rules concerning pollution, marriage, and casual contact.[2] Similarly, partly for security, partly from preference, whenever British men (especially when British women joined them) settled semipermanently in Asia or Africa, they formed distinct residential areas designed to maintain physical and psychological distance between a ruling race and subject peoples.

The practice of segregation was therefore old, and it continued. But the doctrine of segregation came into vogue only at the very end of the nineteenth century. As I argued at length in a study of South Africa and the American South, the ideology of segregation rose at precisely the time when the forces shaping economy and society were bringing peoples closer together. Segregation was associated not with reactionary attitudes inherited from slavery or the frontier but with phenomena ordinarily regarded as progressive: cities, factories, sophisticated communications, modern political systems. And it was first developed by those usually labeled as moderates or liberals.[3]

The closest the *OED* comes to a current definition of segregation is a quote from the *British Medical Journal* (1904): "Manson has also declared segregation to be the first law of hygiene for Europeans in the tropics." For this usage does not refer to the isolation of particular contagious individuals. Here, instead, segregation is a class term. On medical grounds, Europeans were advised to segregate themselves from those black, brown, and yellow peoples who lived in the tropics.

In a recent and valuable article, "Medical Knowledge and Urban Planning in Tropical Africa," Philip Curtin explored an important conjunction—to which Manson's statement is a clue—that took place in the first decade of the twentieth century between the rapidly growing knowledge of tropical diseases, especially malaria, and the rise of town planning in the cities of tropical Africa.[4] In Curtin's

[2] The literature on caste is vast. See, particularly, Louis Dumont, *Home Hierarchicus: The Caste System and Its Implications*, trans. Mark Sainsbury (Chicago, 1970).

[3] John W. Cell, *The Highest Stage of White Supremacy: The Origins of Segregation in South Africa and the American South* (New York, 1982).

[4] See Philip Curtin, "Medical Knowledge and Urban Planning in Tropical Africa," *AHR*, 90 (1985): 594–613. Although I believe the tone of my own article will probably make any additional explanation redundant, I want to emphasize that I have long been an admirer of Curtin and remain so. I heard an earlier version of his article, which he gave at Duke University as a lecture commemorating Gerald Hartwig, my late colleague in African history and a close friend. Because I had been interested for some time in the origins of segregation, I asked Curtin to send me a copy of his revised paper, which he very graciously did. His arguments and footnotes directed me very clearly to the main questions and sources. When I examined those, however, it seemed to me that they did not square with the part of his analysis on India and, especially in view of the importance of his article, that a revisionist rejoinder should be published. Finally, I sent a draft of this article to Curtin and have profited from his corrections and criticisms. I guess that is the way the research process is supposed to work.

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view, the origins of this conjunction can be traced primarily to British-Indian sources. In the early 1860s, he argued, confronting the necessity of maintaining large numbers of British troops as a safeguard against a recurrence of the Indian Mutiny, the Royal Commission on Army Sanitation made numerous recommendations on decreasing the appalling number of deaths from malaria among British soldiers. These included the maintenance of sufficient distance (say, one-quarter to one-half mile) between military cantonments and large concentrations of Indians. Following these recommendations, military and medical authorities managed to reduce mortality rates from over fifty per thousand in the half-century before 1857 to less than sixteen per thousand by the early 1890s, before the germ theory of disease had made any significant impact on medical theory and practice. Meanwhile, Curtin wrote, in the European quarters of cities (including the so-called civil lines, areas where European officials lived) as well as in the military cantonments, the British consciously increased their physical and psychological distance from Indians, developing a pronounced pattern of racially stratified urban communities. In Africa after 1900 this half-century of medical theory and practice was brought to a logical conclusion. The spectacular growth of medical knowledge spurred and justified a development in race relations that was soon regarded as retrogressive. Curtin's article, then, explored the medical origins of segregation.[5]

The drive toward segregated housing patterns in West Africa, he continued, was closely connected with the discovery of the etiology of malaria. Its name derived from the Italian words for "bad air," malaria had been supposed a miasmatic disease originating in the environment—from poisoned soil, water, or air—activated perhaps by meteorological circumstances, such as wet periods followed by rapid drying under hot tropical suns. So long as the miasmatic theory held, if Europeans lived in malarious districts, it seemed that not a great deal could be done to protect them from infection. In 1880, however, the French doctor Charles Laveran identified in Algeria a malarial parasite. Was the parasite causally connected with the disease? Was it the only cause? If so, how did it enter and leave the system of the human body? In the early 1890s, Dr. Patrick Manson, working on the analogy of his own research on the role of mosquitoes in spreading encephalitis, suggested that malaria was probably spread by insects, possibly through their infection of the water supply. A breakthrough came later in the decade when Ronald Ross discovered that the parasite passes through its sexual phase in the stomachs of the females of certain species of *Anopheles* mosquitoes and its asexual phase in humans. His research was soon confirmed by Giovanni Grassi (Ross's rival) and others.

[5] Also see Raymond E. Dumett, "The Campaign against Malaria and the Expansion of Scientific Medical and Sanitary Services in British West Africa," *African Historical Studies*, 1 (1968): 153–97; and Thomas S. Gale, "Segregation in British West Africa," *Cahiers d'études africaines*, 80 (1980): 495–508. In my view Gale regarded the medical grounds for segregation much too uncritically. For the relation between disease and urban segregation in South Africa, see Maynard Swanson, "The Sanitation Syndrome: Bubonic Plague and Urban Native Policy in the Cape Colony, 1900–1909," *Journal of African History*, 18 (1977): 387–410.

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By 1900, then, the basic facts about malaria were known.[6] Its cause is indeed a parasite, which occurs in two hosts—both necessary for its survival—and nowhere else. As had long been known, quinine is effective against the disease, as a curative and as a prophylaxis. Two strategies were available, and each had its passionate adherents: attack the parasite in humans or strike at *Anopheles* mosquitoes, either in the adult or larval stage.

The linkage between segregation and medical knowledge, Curtin argued, was still more specific. In 1900 the great German microbiologist Robert Koch, who along with Louis Pasteur had pioneered the germ theory of disease, published the results of research on indigenous children living in highly malarial regions. Unlike adults, who seemed gradually to acquire a degree of immunity, children provided fresh reservoirs of infection. Later that same year, the Royal Society of London sponsored malaria expeditions to West Africa, led by Drs. J. W. Stephens and S. R. Christophers, who concluded that Ross's scheme to exterminate *Anopheles* was impracticable. During dry seasons a degree of temporary success might be achieved by sprinkling kerosene on breeding places. In the wet seasons, however, when little pools capable of producing thousands of mosquitoes were almost infinite, the insects returned as numerous as ever. It was also not practical, the Stephens-Christophers reports concluded, to attack the parasite in Africans, especially in children, its continually replenished reservoir. For Europeans, it seemed, only one strategy was available: separation from Africans. Wherever possible, Europeans should build their houses on hills, or at least raised on stilts, at a distance of one-quarter mile or more from African towns and villages. By means of screens and nets, Europeans might try to keep from being bitten. They might take prophylactic doses of quinine. But the danger was not being bitten; it was being bitten by a female *Anopheles* that had previously fed on an infected person. Most important, therefore, sleeping quarters of Europeans should be separated from those of Africans, especially of African children. If these simple and practicable measures of segregation were carried out, the reports concluded, Europeans could live in the tropics in safety and in health.[7]

Although the so-called scramble for Africa is ordinarily associated with the 1880s, the effective occupation of key stations in its tropical regions by Europeans occurred only in the years around 1900—and then only by small numbers of them. Largely in an attempt to make the exploitation of Africa feasible by enabling Europeans to live there (especially with families), research into tropical diseases accelerated in all the colonial countries, led in Britain by new schools of tropical medicine at the Universities of London and Liverpool. Moreover, the Royal Society's malaria expeditions coincided with drives toward segregation that were already occurring in Sierra Leone, the Gold Coast, and parts of Nigeria, though not in Lagos, whose governor, Sir William MacGregor (himself a doctor), took the

[6] For the best general book on malaria, see Gordon Harrison, *Mosquitoes, Malaria, and Man: A History of the Hostilities since 1880* (New York, 1978). For a current medical textbook, see Leonard J. Bruce-Chwatt, *Essential Malariology* (London, 1980).

[7] J. W. Stephens and S. R. Christophers, "The Segregation of Europeans," in Royal Society of London, *Reports to the Malaria Committee* (London, 1900–03).

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enlightened view that the primary aim of tropical medicine should be to treat people who lived in the tropics—that is, Africans—and that the employment of African taxes to develop luxurious segregated areas for Europeans was indefensible. Europeans, he argued, ought to set an example by using nets and screens. Like Ross, however, MacGregor made his primary goal the eradication of mosquitoes.[8]

Curtin examined in detail the varying responses to segregation in the several British colonies in West Africa. In the first decade of the twentieth century, governors like MacGregor were rare and out of favor. The growing trend toward segregation, justified on medical grounds, reached its climax and logical conclusion in the famous *Political Memoranda* by Sir Frederick (later Baron) Lugard. In all African towns, he directed, European reservations should be set aside, with their exclusive clubs and athletic facilities, all to be "surrounded by a non-residential area 440 yards broad." Within these European zones, "no Natives except the *bona fide* servants of Europeans" would be allowed to live. The European areas were to be laid out very precisely: 100 yards deep, 70 to 100 yards in front, "enclosed by a live hedge, mud wall, or substantial fence." Trees would be planted, their lower branches being carefully pruned to a height of 6 feet, while "servants' quarters [would] be at least 50 yards in rear." The governors of Sierra Leone and the Gold Coast agreed that "the first object of the non-residential area is to segregate Europeans so that they shall not be exposed to the attacks of mosquitoes which have become infected with the germs of malaria or yellow fever, by preying on Natives, and especially on Native children, whose blood so often contains these germs"—unless, of course, the infected Africans happened to be bona fide servants. The belt of land would guard against fire. And "it removes the inconvenience felt by Europeans, whose rest is disturbed by drumming and other noises dear to the Native."[9]

Notwithstanding the euphoric mythology that later surrounded Lugard, as he came to be regarded as *the* British administrator in Africa, his ideas were in fact too extreme to be typical, and they were never fully carried out. Africans, who were represented in legislative councils, naturally agreed with MacGregor, not least in regard to the use of their taxes. Measures against infection ought to aid them, not simply protect Europeans by surrounding them in cotton wool. Sir Hugh Clifford and other governors gradually swung against what in retrospect appears to have been a passing fad.[10] Segregation as a doctrine went out of vogue, although interesting and important linkages continued between segregation and indirect rule, ideas about African education, and even African nationalism. But segregated urban housing patterns remained, ever resented by Africans. As John Flint has

[8] William MacGregor, "Notes on Antimalarial Measures now being taken at Lagos," *British Medical Journal* (hereafter, *BMJ*), September 14, 1901. MacGregor's article was derived from his part of a panel discussion that also included Ross. For MacGregor's program and the Colonial Office's hostile reception, see Roger Joyce, *Sir William MacGregor* (Melbourne, 1971), esp. chap. 10.

[9] Frederick D. Lugard, *Political Memoranda: Revision of Instructions to Political Officers on Subjects Chiefly Political and Administrative, 1913–1918*, ed. Anthony Kirk-Greene (1919; reprint of 2d edn., London, 1970), 416–18.

[10] Curtin, "Medical Knowledge and Urban Planning."

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argued, it would have been better if Europeans had simply and honestly said that they preferred to live apart from Africans.[11]

The important and paradoxical conjunction that occurred in Africa between an accelerating knowledge of tropical diseases and a reactionary policy in race relations owed very little to the Indian influence. Again, theory must be distinguished from practice. The British also developed segregated housing and social patterns in India, but they did not ordinarily justify these on medical grounds. Nor did medical theory much influence the distribution and stationing of British troops.

The medical justification for segregation as a protection against malaria emerged quite suddenly in 1900 in West Africa, precisely in the way that Curtin explained. But it was an essentially new idea. Only afterward was the proposal made in India—as it happens by the very group from the Royal Society of London that first advanced the notion in Africa. In India the British had long maintained separate residential quarters, gymkhana clubs, churches, and so forth, all of which their town plans perpetuated. But the fad of justifying a doctrine of segregation primarily on medical grounds had an even shorter run there than it did in Africa—hardly, indeed, a run at all.

TWO COMPARATIVELY RECENT IDEAS—that soldiers did not have to die in huge numbers from causes unconnected with battlefield injuries, as they always had, and that mortality rates might be substantially decreased through sanitary measures—are usually associated in Britain with the campaign led by Florence Nightingale and Sydney Herbert during the Crimean War. But conditions more scandalous than those in the Crimea soon appeared in India. For, after the mutiny of the bulk of the Sepoy army of Bengal in 1857 (and despite the fact that loyal troops from the Punjab had already largely defeated the mutineers), large numbers of British troops were sent out to mop up the remaining rebels, to demonstrate Britain's resolve to hold India, no matter what the cost. As it turned out, the cost—from a variety of diseases including cholera, dysentery, liver abscess, enteric (typhoid) fever, and malaria—was appalling. In 1858, ten men in one hundred died, the chances being only four in one hundred that they would die from battlefield injuries. The government established the Royal Commission on Army Sanitation in India, which examined numerous witnesses, compiled voluminous statistics, visited the country for on-site inspections, and at length made recommendations.[12]

The sanitation commission concluded that the health of Europeans must in the long run deteriorate in the debilitating climate of India's plains. It was by no means inevitable, however, that soldiers in India die at an average rate of seventy per thousand (compared with the current mortality rate of seventeen per thousand at home), as they had in the half-century before the Mutiny. From every regiment a company had been lost every twenty months: "An army of 70,000 British in India

[11] See John E. Flint, "Nigeria: The Colonial Experience, 1880–1914," in Lewis Gann and Peter Duignan, eds., *Colonialism in Africa*, 5 vols. (Cambridge, 1969–), 1: 220–60.

[12] *PP*, 1863 [3184], p. 19, Royal Commission on Army Sanitation in India.

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has, so to speak, a vast hospital of 5,880 beds constantly full of sick, and loses yearly by death 4,830 men, or nearly five regiments."[13]

Although the sanitation commission conceded that the climate might have long-term, debilitating effects, climate was not directly responsible for such huge losses. The soldiers died from specific diseases, the causes of which were as yet unknown. "Worthy of remark," however, was "that nearly all . . . are accompanied by profuse discharges, with which the air, water, bedding, linen, closets, walls of hospitals and barracks become more or less infected. In a common building the *materia morbi* comes into contact with all the inmates."[14] One day scientists might supply more precise answers about causes, prevention, and treatment. Meanwhile, common sense as well as prevailing medical theory suggested that these illnesses be regarded as filth diseases. The commission's report stressed four preventive measures: ventilation—so much space per man; proper sites for barracks, not in such notoriously malarious places as marshes or river banks but, wherever possible, on the highest available ground, and the use of only upper stories for sleeping; good drainage, requiring careful inspection of the soils and especially subsoils; and proper sanitation, especially in handling food and water supplies, as well as conservancy, that is, disposal of waste. Finally, the commission argued that, whenever militarily and politically possible, hill stations should be used more frequently, serving as relief stations for whole regiments as well as sanitaria for sick individuals. Were such simple measures carried out, the mortality figures for British soldiers in India might be brought down to those for their colleagues at home.

Among the proposals considered by the sanitation commission was one in favor of maintaining a considerable distance between cantonments and the surrounding Indian population, making them secluded enclaves. According to Dr. W. J. Moore, the author of a standard work on Indian disease, the ideal cantonment "would be placed at a distance of some miles from and to windward of both large rivers and cities," preferably "on the summit of a gentle undulation, or on the slope of a rising ground, not sufficient to overhang the station, to intercept the breeze or to radiate heat. The surface would be light, friable, or gravelly soil, without a substratum of heavy, retentive material." Ravines should be avoided, both because the wind blew malarial poison up them and because Indians (who, after all, had no alternative) frequently employed them to answer calls of nature. Irrigation tanks and canals, equally likely to be polluted, should also be shunned. "The whole cantonment would front the prevailing winds; native houses, bazaars, burial-grounds, places of Hindoo cremation,"—indeed, all essentials except wells would be to the rear. "The station should be surrounded by a zone of one or two miles radius, free from cultivation and irrigation"—and Indians.[15] Charles A. Gordon, surgeon-general

[13] *Ibid.*

[14] *Ibid.*

[15] W. J. Moore, *Health in the Tropics; or, Sanitary Art Applied to Europeans in India* (London, 1862), 118–19. Moore was the author of *A Manual of the Diseases of India* (1861; 2d edn., London, 1886) and *A Manual of Family Medicine in India* (London, 1874), the latter commissioned by the Government of India especially for use by those unable to consult a doctor. Both were standard works, reprinted in several editions.

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of the then-separate Army of Bengal, gave a similar description. "Stations should be surrounded by a zone of one or two miles radius free from cultivation and irrigation. This [cordon] should be dotted with clumps of trees, and be for the most part grass-land." Malarious districts, especially those close to river banks or marshes, should be avoided, as well as the "vicinity of large cities, especially to leeward of them, also the proximity of cantonments occupied by the native black troops of India, whose habits put all sanitary rules at defiance."[16]

Such descriptions of ideal military stations, which are strikingly like those of Lugard half a century later, also appear to correspond closely with the patterns of racial stratification that the British developed and eventually left behind them in Indian cities. "You find by degrees," wrote the *Times* correspondent William H. Russell in a classic, if misleadingly simplistic, depiction in 1858, "that an Indian station consists of two parts: the cantonments of the Europeans, the native city and bazaar. The west and the east end are far apart, separated by a waste common, or by fields, or gardens. . . . There is no bond of union between the two, in language, or faith, or nationality."[17] He noticed in the European cantonment—and, as was common among the British in India, he meant the whole European section of the town, not a strictly military camp—that the houses were all neatly laid out on wide and shaded streets, consuming four times as much space as did those of the Indians.[18]

Two decades after independence, Indian cities still showed the old division. "Going from the cantonment bazaar which is still the fashionable shopping centre of Mayapore," wrote Paul Scott in the mid-1960s, again using cantonment in a nonmilitary sense, "along the Mahatma Gandhi road, once styled Victoria road, the traveller will pass the main police barracks on his left and then, on his right, the Court house and the adjacent cluster of buildings, well shaded by trees, that comprised, still comprise, the headquarters of the district commission." Just beyond were the bungalows of the assistant district commissioner and superintendent of police, now brown-skinned Sahibs. "A quarter of a mile farther on, the Mahatma Gandhi road meets the south-eastern angle of the large open space known as the *maidan*, whose velvety short-cropped grass is green during and after the rains but brown at this season. . . . Along Club road you arrive eventually at the Gymkhana. . . . And it is along Club road, facing the *maidan* that the bungalow of the Deputy Commissioner is still to be found, in walled, arboreal seclusion."[19]

In fact, in military cantonments, no such wholesale separation between British soldiers and Indians occurred. Nor was the separation that did exist ordinarily justified on grounds of medical policy. Instead, the prevailing rationale was military security. Although the idea of protecting British troops by isolating them in sanitary enclaves was not reserved for cranks, as the standing of doctors such

[16] Charles A. Gordon, *Army Hygiene* (London, 1866), 126–35.
[17] Russell, *My Diary in India, in the Year 1858–59*, 2 vols. (London, 1860), 1: 180.
[18] On British town planning in this period, see Veena T. Oldenburg, *The Making of Colonial Lucknow, 1856–1877* (Princeton, N.J., 1984).
[19] Paul Scott, *Jewel in the Crown* (1966; New York, 1979), 160–61.

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as Moore and Gordon makes clear, it was by no means a majority opinion.[20] Nor did it become official policy. For the Royal Commission on Army Sanitation did not repeat or endorse Moore's description of an ideal cantonment. Its recommendations contain no explicit instructions about the distance that should separate cantonments from Indian towns, or about removing Indian villages in the vicinity. Indeed, the commissioners assumed that the elimination of contact between British soldiers and the people among whom they served was not a practical policy, a conclusion that the large and growing statistics on venereal disease certainly bore out.[21] On the contrary, they maintained, the health of British troops was ultimately inseparable from that of the people of India. Conditions in the great entrepôts such as Calcutta, Bombay, and Madras, in large towns in the mofussil such as Lucknow and Kanpur, in the countless villages where the bulk of the people lived, even in the hill stations whose use the commissioners so strongly recommended—all were a sanitarian's nightmare. Typically, epidemics began among Indian communities before spreading to the cantonments. Since epidemics were caused or fed by filthy environments—air, water, soil, food, conservancy—it followed that those conditions must be attacked.

The sanitation commission strongly recommended publication of annual reports to be compiled by a permanent sanitation department under the Government of India. Actually, for most of the period (after 1874) there are two sets of annual reports, both in the British *Parliamentary Papers*. One set concerns the health of troops, both British and Indian; the other is a series of general reports on sanitation. Together they constitute a voluminous and valuable record of the growing concern with public health in Britain and its extension to the empire. The commentaries, which until his death in 1879 were compiled by E. A. Parkes, professor of hygiene at the Army Medical School at Netley and the author of a standard work on military hygiene, also provide a chronological record of the gradual and then, in the 1890s, accelerating transition from the miasmatic to the germ theory of disease.

For several reasons, cantonments (as well as jails, which mainly contained Indians) were to some extent treated as sanitary enclaves. Excluding the fifty million residents of native states, the population of British India was about one hundred fifty million, and financial resources were extremely limited. Cantonments were comparatively small and therefore capable of being closely regulated, and residents could readily be used as control groups. Until late in the century,

[20] Segregation of British and Indian soldiers was not mentioned in the most standard contemporary work on army hygiene by Edmund A. Parkes, professor at the Army Medical School at Netley. See Parkes, *A Manual of Practical Hygiene*, ed. J. Lane Potter (8th edn.; London, 1891). Nor is it found in Norman Chevers's *A Commentary on the Diseases of India* (London, 1886) or Joseph Fayrer's *On the Climate and Fevers of India* (London, 1882). The concept also does not appear in the Groonian lectures of the Royal College of Physicians or in Patrick Manson's *Tropical Diseases: A Manual of the Diseases of Warm Climates* (London, 1898)—in its revised form (now the 18th edition) still the standard general work on the subject. I have not made an exhaustive search of the Anglo-Indian medical literature, but I have no doubt that examples of the enclave approach could be found. The point that clinches my argument, however, is that it is never once advocated in the annual medical or sanitation reports.

[21] See Kenneth Ballhatchet, *Race, Sex, and Class under the Raj: Imperial Attitudes and Policies and their Critics, 1793–1905* (London, 1980).

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the most elementary statistics on births and the numbers and causes of deaths were unreliable, even in cities. In rural areas they were simply unavailable. Moreover, the Government tended to shrink from interfering too vigorously in the lives of Indians. For that, as the violent reaction against the anti-plague measures in Bombay in the late 1890s showed, could be extremely dangerous. Thus, many measures were applied to cantonments that did not reach the surrounding towns and villages. But the assumptions that the health and sanitary conditions of troops were ultimately inseparable from those of Indians and that the medical and sanitary departments of the Government of India ought to serve the people of India were never abandoned. Like the sanitation commission, the new sanitation department did not advocate segregation.

If it had, it would have sharply contradicted military realities and political objectives. In the first place, British troops did not really live widely apart from Indians. According to the *OED*, "cantonment" came into common usage during the eighteenth century, gradually replacing "fort" as the term for permanent encampments in India. Also gradually, as increasing numbers of civilians and especially women lived semipermanently in India, "cantonment" lost its strictly military connotation.[22] Administratively, however, again in a military sense, it meant not merely the barracks of soldiers but a comparatively large area under the control of military authorities. Under a policy developed after the Mutiny, the number of British troops was supposedly never to fall much below a proportion of one British for every two Indian soldiers (a proportion that was never reached). Except on a few frontier outposts, all artillery was to be in the hands of Europeans.[23] The bulk of the army continued to be Indian, which the authorities carefully attempted to separate according to region, religion, and caste. Military strategy dictated that artillery, cavalry, and infantry be kept together. Moreover, the primary functions of the British troops were to prevent mutinous combinations from occurring or spreading among the Indian portion of the army and to overawe civilian populations and lead in suppressing uprisings when they did occur.

Thus, a typical cantonment contained barracks for Indian as well as British troops, with the former ordinarily in the majority. Further, British troops were utterly dependent on huge numbers of Indian followers—grass-cutters, grooms, cooks, sweepers, punkah-wallahs—without which no army could survive much less fight.[24] Each cantonment had its own bazaar, which, in addition to being a supply depot, provided shops and services of all kinds, adding to the number of Indians. Finally, cantonments often contained villages that predated the camps, whose

[22] The word "cantonment" grew out of the verb "canton," meaning to divide, as in the Swiss cantons. The military meaning was "to quarter [soldiers]; to provide with quarters." Also see Henry Yule and A. C. Burnell, *Hobson-Jobson: A Glossary of Colloquial Anglo-Indian Words and Phrases* (London, 1903): "This English word has become almost appropriated as Anglo-Indian, being so constantly used in India, and so little used elsewhere." The references are to the eighteenth and early nineteenth centuries.

[23] *PP*, 1859 [2515, Sess. 1], vol. 5, [2541, Sess. 2], vol. 8, Select Committee on Organization of the Indian Army (hereafter, Peel Commission).

[24] Russell, *My Diary in India*, 2, 345–46.

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residents possessed land rights difficult and expensive to abrogate, or new settlements populated by unofficial followers, including prostitutes. The sanitary department inspected cantonments, bazaars, and villages routinely.[25] During epidemics, infected cities were declared off-limits. But restricting the contact between British soldiers and Indian civilians could go only so far. And, since the number of Indians inside cantonments exceeded the number of Europeans, constructing wide buffer zones around them, after the manner of Moore's ideal description, would not have been terribly logical. Nor was a rigid segregation of British troops from Indians compatible with the assumptions and aims of political and military strategy. European troops were there to occupy and hold a country that, especially in the years immediately after the Mutiny, the British assumed was hostile.[26] Unfortunately, from the sanitarian's standpoint, the army and people of India happened to be on the plains. To be of any use, the bulk of the British army had to be there, too.[27]

Throughout the nineteenth century and indeed beyond, several key questions dominated the thinking of the Government and army of India. Should the native army be kept weak and divided, thus more easily overpowered at the first sign of mutiny? Or, in the interest of military efficiency, should it be made strong, provided with a unified administration, and equipped with breech-loading rifles and other modern weapons? Given the illusive target of a two to one ratio, the native army's size was dictated by the number of British troops for which the Government of India could pay, their cost (including transport, better facilities, and higher mortality rates) being roughly three times that for Indian troops. Should British troops be massed and concentrated, so that they could be brought to bear quickly and overwhelmingly at particular points? Or should they be dispersed, the better to keep the British presence and resolve, as one general put it, "palpably *en evidence*"?[28] How far were these considerations compatible with the increased use of hill stations?

[25] See, for instance, the report on the village of Bhingar, near the town of Sholapore in Bombay Presidency and one-half mile from British barracks. It was a "filthy village," containing both a tannery (and therefore untouchables) and what the report called the worst sort of prostitutes. Medical officers had long complained about the town, and a recent cholera epidemic was said to have begun there. Proposals had been made either to remove the village or to incorporate it within the cantonment, but investigators thought neither was practical. Instead, they recommended the appointment of a special European magistrate to direct a cleanup operation. *PP*, 1866 (529), vol. 53, Bombay Sanitary Commission Annual Report, 1865. A reasonable guess would be that, supposing such a magistrate had indeed been appointed, a few steps would have been hastily taken, after which the magistrate would have departed, whereupon the village would have returned to normal.

[26] For a study of the local impact of this mentality, see Oldenburg, *Making of Colonial Lucknow*, esp. 27–61.

[27] Although Curtin cited as primary sources the Army Medical Department's annual reports, for the role of medical theory in framing army policy on the siting of cantonments he relied on a study by Anthony D. King. See Curtin, "Medical Knowledge and Urban Planning"; and King, *Colonial Urban Development: Culture, Social Power, and Environment* (London, 1976), 97–122. King's book is extremely interesting and suggestive, especially on the importance of words such as "cantonment" as clues to attitudes toward the use of space. The only source that King cited on the segregation of troops, however, is the selection I have quoted from Moore's *Health in the Tropics*. King cited the report of the Royal Commission on Army Sanitation in India on another point, and he did not appear to have used the annual medical or sanitation reports.

[28] Minute of General E. B. Johnson, March 4, 1880, *PP*, 1884–85 (17), vol. 59, Papers re Indian Army Reorganization. The implication that troops must be distributed to pose "a constant threat to all classes

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In the late nineteenth century, the British gradually regained confidence—first, in the loyalty of the Indian army and, second, in their own ability to form effective alliances with important Indian groups, especially in rural areas in regions such as the Punjab, so important for military recruiting. The two to one ratio was not observed. After flirting with the concept of equipping native soldiers with outdated weapons, the British supplied them with breech-loading rifles. That the Indian army continually fell behind in training, equipment, and administrative efficiency, which greatly hurt its performance in both world wars, was largely the result of straitened finances, not deliberate policy.

The distribution of British troops was primarily dictated by political considerations and military realities, especially changes in communications. One officer recalled that, when he had first gone to India in the 1840s, it had taken him three months to get from his embarkation point at Calcutta to the Punjab, where the army was then at war with the Sikhs.[29] By the 1860s, the railroad had shrunk that to a week. Only when connected to the rail lines, at great expense, did hill stations become practical for more than handfuls of troops. Some administrators in England, even Financial Members of the Viceroy's Council, were inclined to believe that with improved communications the size of the army, and therefore of its extremely expensive British component, could be substantially reduced. Not so, the commander-in-chief and other authorities always replied, predictably maintaining that the army was not a man too large. Troops might indeed move by train, if the enemy were thoughtful enough to fight near a railroad (and not until the 1880s did the railroad reach the frontier). But moving big guns by rail, as well as the large numbers of elephants and camels essential for an Indian campaign, was prohibitively expensive. Besides, the railway, especially key stations, tunnels, and bridges, needed protection against internal sabotage; in effect, the network was yet another long frontier.

Moreover, though within a decade after the Mutiny few people argued in favor of a dispersal of troops, certain key points had to be occupied by British soldiers. Being too concentrated could be dangerous. Before the Mutiny, one colonial administrator wrote, "we had scarcely a place on this [western] side of India deserving of the name of a fortification, where we could put our women and children, our treasure, and our munitions of war, when our troops took the field." Those few they had possessed, he continued, had ordinarily been garrisoned by Indian troops. The Mutiny proved the value of such places, as well as the folly of not putting British troops in them. At great cost they had had to be reconquered. "Then every man felt the necessity of having places of refuge, fortified positions . . . to guard [against] treachery and sudden onslaught."[30] Such redoubts had to be located near cities—though on grounds of security not too near—because the

of the Queen's Native subjects" received a withering response from Lord Lytton, the viceroy, who found "it impossible to understand, and therefore difficult to deal seriously with, such reasoning as this." Minute of May 16, 1880, *ibid.*

[29] Minute of Major-General Alexander Fraser, May 27, 1880, *ibid.*

[30] Lawrence, as quoted in Krishnan M. Saxena, *The Military System of India, 1850–1900* (New Delhi, 1974), 161–65.

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families of British soldiers, merchants, civil servants, and missionaries also lived in those cities.

Although the nervousness of colonial officers gradually receded, the need to select cantonment sites for British troops close to key strategic points continued. In view of changing circumstances, especially in communications, no hard and fast rule could be followed. Distribution plans had to be made and revised province by province, district by district: so many British and Indian infantry and cavalry battalions here, so many artillery units there. Not unmindful of their troops' health, the authorities abandoned unhealthy stations whenever they could. Marshy areas were avoided—except when, as at Calcutta or Chittagong, those marshy areas included capital cities or ports. River banks were also malarious. But Indians dependent on river traffic had built important cities near them, such as Delhi or Lucknow, so those, too, had to be occupied. In irrigated areas, which the British were themselves extending in an effort to increase agricultural production, such stations as Mian Mir (where the Royal Society's malaria experts conducted their tests in 1902–04) were notoriously unhealthy. Were the thriving canal colonies of the West Punjab, populated largely by the valuable but dangerous "martial races," therefore to be left entirely to themselves?

By the time administrators finished listing such key points, not much flexibility remained. Gradually, taking advantage of the railway, improving roads, and ultimately the internal combustion engine and the airplane, the Indian army in general, and the British contingent in particular, did consolidate. Yet from Dalhousie's scheme of 1856, which predated both the Mutiny and all but the beginnings of the rail system, to the end of Britain's occupation of the Indian subcontinent in 1947, the number of key points that had to be guarded—and by British troops—remained remarkably consistent.[31] In 1867 an India Office witness testified before a select committee that the recommendations of the Royal Commission on Army Sanitation had had virtually no effect at all on the distribution of British troops in India.[32] Furthermore, the commission's recom-

[31] Compare the successive army distribution plans: memorandum by Dalhousie, February 5, 1856, printed as an appendix to Select Committee on British Army in India, *PP*, 1867 (478), vol. 7; memorandum by Sir John Lawrence, Peel Commission (Appendix), *PP*, 1859 [2541, Sess. 2], vol. 8; and Report of Army Reorganization Commission, 1879, *PP*, 1884–85 (17), vol. 59. In the 1890s the Indian army was again reorganized, being placed under the unified command of a commander-in-chief, thus destroying the individual Presidency armies. See *PP*, 1893–94 [C. 6987, 7009, 7115], vol. 63, Further Papers re Proposed Changes in the Indian Army System. But neither that change—the controversial Kitchener reforms in the early 1900s that eventually forced Curzon's resignation—nor the proposals of the Esher Committee after World War I altered the general scheme of distribution of British troops. Finally, see Great Britain, Public Record Office, C. P. 133 (39), Cab. 24/287, Report of the Expert Committee on the Defence of India (1939). The main difference between earlier schemes and the proposals made in 1939 is that the Expert Committee on the Defence of India was concerned with coastal defense. Nineteenth-century plans were based on the assumption that British naval supremacy east of Suez would always be sufficient. The 1939 scheme stressed the protection of particular arms factories, oil storage depots, bridges, and airfields. Finally, the report felt "bound to assume" that the loyalty of the Indian army could be relied upon, that is, if the entire native part of it should mutiny, the British component would be unable to put it down. But the strategic points requiring defense by British troops in case of civil insurrection remained much the same as those in Dalhousie's scheme of 1856.

[32] The proposal that one-third of British troops in India should at all times be in the hills, testified Colonel Henry Norman, the military secretary to the Government of India, "was a recommendation

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mendations never dictated political and strategic thinking.[33] The use of hill stations increased, not least as a result of the Government's annual migration to them during hot weather. But the army's primary mission as well as its weapons and systems of communication remained on the plains.

The annual reports provide abundant evidence that wide cordons between European troops and Indian barracks, cities, and villages existed infrequently if at all. The military authorities would have preferred to have them, of course, not only because they were presumed to protect health but also because they would have provided areas where syces might gather the huge amounts of grass the army required, instead of having to take (or steal) it from peasants.[34] From Coloba (Bombay), Dum Dum, Barrackpore (Calcutta), and Secunderabad (near Hyderabad), from Agra, Lucknow, and Delhi came frequent complaints about unsanitary conditions in nearby cities, bazaars, and villages. India had been a densely settled country for centuries. Arable land not already under cultivation was scarce; areas that did remain vacant were ordinarily hills, deserts, or jungle. And land only recently cleared, declared the experts, was the most malarious of all. Moreover, both the areas near the railroads and the troops (who had to be near the rail lines) offered economic opportunities to Indians who naturally tended to flock to the cantonments. Under these conditions, British authorities could not implement the recommendations of the sanitation commission.

THUS, DESPITE THE GRADUALLY INCREASING USE of hill stations, the Indian medical service had primarily to continue to treat and protect the troops on the plains. In fact, argued members of the sanitation department defensively, although soldiers stationed in India were certainly at greater risk than those at home or in temperate colonies, the sanitation commission's mortality statistics had been unduly grim. The average of seventy per thousand compiled for the period 1800–56 was misleading. For the statistics covered numerous campaigns of conquest—against the Mahrathas and the Sikhs, in Burma and in Sind—in which the number of casualties on and off the battlefield had inevitably been greater. It was as though the health of the contemporary French army were being analyzed on the basis of the Napoleonic era, including the disastrous retreat of 1812 from Moscow. The largest number of deaths, moreover, had occurred in years well before the growth of interest, even in Britain, in the health of armies. Most of the experts who testified before the commission had left India a decade or so earlier and were unaware of recent improvements.[35]

Averages, concluded the first annual report of the Bengal Sanitary Commission for 1864–65, were misleading anyway. For they tended to mask the single largest

of the sanitary commission, who had not before them the political and military considerations which have to be taken into consideration in India." In the early 1860s, the authorities had concluded that no more than three to four regiments could be spared for the hills. *PP*, 1867 (478), p. 7, Select Committee on British Army in India.

[33] See Saxena, *Military System of India*, 163.

[34] *PP*, 1884–85 (17), vol. 59, Report of the Commission of 1879.

[35] *PP*, 1866 (79), vol. 53, Bengal Sanitary Commission, 1st Annual Report (1864–65).

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cause of death—cholera. In some parts of India the disease was endemic, never disappearing, always increasing during the monsoon season after the hot weather, though not always to epidemic proportions. Some years were cholera years; others were not. Among troops the disease was epidemic. Annual mortality rates from all causes for the years immediately before the Mutiny ranged from a low of 40 per thousand in 1855–56 to a high of 71.5 in 1856–57, the latter a cholera year. For the turbulent last nine months of 1857, continued the Bengal commission report, no figures existed by which battlefield and nonbattlefield casualties could be distinguished with any confidence. But for 1858 mortality was an appalling 100.6 per thousand, of which only 4.3 deaths per thousand happened as a result of military action. The figures explained why the British public had become so alarmed, but, fortunately, they were hardly typical. Many of the sanitation commission's recommendations had been anticipated, and much had been done after its report in 1858. Between 1859 and 1865, the mortality rate had not gone above 46 per thousand, which was in 1861, when perhaps the worst cholera epidemic in modern Indian history occurred. In other years, the figures were 28 (1862), 25 (1863), and 21 (1864) per thousand. Except for cholera years, the rate for British troops in India was not much higher than for those at home.

To serve in India was still dangerous. Much more needed to be done, especially in the cities and villages where endemic diseases periodically became epidemics. "Even if we look no further than the protection of the health of the European soldiers," the first Bengal sanitary report explained, "it will be evidently insufficient if we endeavour to improve the sanitary condition of our cantonments alone, and ignore the existence of the masses of the native population by which our troops are surrounded."[36] In graphic terms the report proceeded to describe the incredible cesspool otherwise known as Calcutta, the second largest city of the British empire. Conditions in the villages were perhaps even worse, but no statistics were available. So far as the narrower question of the health of troops was concerned, however, the horrible situation described by the sanitation commission seemed to be a thing of the past.

The disease that received most attention in the annual medical and sanitation reports was not malaria but cholera. Between 1860 and 1864, the Bengal report related, deaths from fevers of all kinds (including typhoid and typhus as well as malaria) ranged between a low of 117 (1863) and a high of 237 (1860). But, from cholera, the low was 97 (1864) and the high 1,065 (1861)—out of a British force of some seventy thousand.[37] Vivax malaria, unlike the more virulent falciparum malaria, is on the whole a long-term disease, not an immediate killer, returning sometimes after years in remission or requiring several hospital admissions per year. Moreover, according to the miasmatic theory of disease, not much could be done to prevent it. Either one lived in unhealthy regions or one did not. Some parts of the world—the West Indies, West Africa—were so malarious, the British War Office concluded, that only in extraordinary circumstances would European

[36] *Ibid.*
[37] *Ibid.*

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troops ever live or fight there. For India the decision was different. Britain intended to hold India by force, and, though the bulk of the army would remain Indian, a substantial British component had to stay on the plains. Like the heat, malaria seemed to be the more or less inevitable consequence of that decision.

Until Ross's breakthrough in the late 1890s, malaria remained as mysterious as ever, its causes unknown, a subject for speculation but not for the kind of factual analyses and optimistic forecasts appropriate for annual reports. Not surprisingly, the army medical and sanitation reports allotted more space not only to cholera but also to typhoid fever and venereal disease. Since these appeared to be contagious, the last obviously so, the medical authorities could take steps that would produce results. One could, for example, enact cantonment regulations requiring all "known prostitutes" to receive regular examinations in hospitals.[38] When Koch discovered a cholera microbe in the 1880s, the questions of whether it was a cause or an incidental by-product of the disease and how it was transmitted were still left open, but correlations with impure water, milk, and especially the excreta of cholera patients had already been strongly suspected and studied. Moreover, early in the 1860s, the medical authorities discovered a very effective remedy. Whenever cholera appeared in surrounding villages or cities, they began surveillance; once two or three cases occurred among the troops, they were encamped, that is, removed from the area and housed in tents. More than any other single measure, this step helped reduce the huge mortality among British troops to at least somewhat more tolerable proportions.[39]

Bringing down the mortality rate from cholera still further, especially among ordinary people who could not be encamped at the first sign of danger, would have required a scientific breakthrough. In 1867 the sanitation department launched a full-scale investigation of cholera: its nature, origin, pathology, and modes of treatment. Was it, according to the view prevalent in Europe, contagious, traceable to particular carriers? Was it miasmatic? Or was it both? The department's instructions illustrate the obstacles that confronted medical investigators before the development of the germ theory of disease. They had not a clue where to look. Since a plethora of speculative theories existed, the investigators were to start afresh. Literally everything had to be put under a microscope or be chemically analyzed: air, in particular, varying amounts of ozone and electricity; water, both surface and subsoil; soil, to a depth of several layers; and excreta of cholera patients. The commissioners were ordered to examine the contagion theory systematically, especially by tracing religious pilgrims suspected of being carriers, and gather all sorts of epidemiological and endemiological evidence. How did the mortality rates of Europeans compare with those of Indians, especially those living near cantonments who could be more easily surveyed?[40]

The investigators could not stop there. How, if at all, was cholera connected with general conditions—with "telluric disturbances affecting great areas of country"

[38] See Ballhatchet, *Race, Sex, and Class under the Raj*.
[39] *Ibid.*
[40] *PP*, 1868–69 [4076], vol. 37, Army Med. Dept., AR (1867).

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such as dryness and cracking of the soil or excess moisture in the subsoil, with an unusual rise or fall in the incidence of malaria, with droughts or famines? The scientists were also told to gather data on "a generally disturbed or contaminated state of the atmosphere, marked by unusual heats, colds, winds, calms, clouds, fogs, storms, rainfall."[41] Were simultaneous and possibly related disturbances affecting animals or plants—epizootics or blights? Could anything be learned from correlations with simultaneous epidemics in Mauritius, the West Indies, and the Gulf of Mexico?

Employing such shotgun methods, the patient investigators learned that there was quite a lot of cholera in India, that it was endemic in certain areas (such as Lower Bengal), that it became epidemic during hot and wet periods (in some but not in other years), that it did seem to correlate in some cases, but by no means all, with impure water supplies, and that the poor and hungry were most likely to be afflicted, especially during famines. They also tested the contagion theory, for cholera epidemics at Mecca were notorious, and India swarmed with pilgrims going from shrine to shrine. Yet the disease seemed far too pervasive for pilgrims alone to be responsible, so the investigators tentatively settled for the miasmatic theory. Like malaria and dysentery, they thought, cholera most likely originated from a poison in the soil, from which under certain climatic conditions it rose and spread into the water, into the air—its "germs" perhaps being wafted by dust borne by the hot wind of the plains.[42]

But that could not be right. Encamping the troops did succeed, yet the camps were hardly free of dust. So little had been learned, mused Parkes in his annual commentaries. The endless debates between the advocates of miasma and contagion were strikingly reminiscent of the state of knowledge when he had been conducting his own investigations into cholera epidemics in London—in the 1840s.[43] The truth was probably a combination. "The poison of cholera may apparently have an independent life; it may grow in two or three places, viz., in the ground, in the bowels of men, perhaps in water; it may travel in two or three ways; it must be, in European countries at least, destroyed with great facility, and under favouring conditions grow at times with great rapidity. Like many lower organisms it can become dormant and again revive."[44]

As the germ theory of disease began to gather support in the 1880s, some of the old miasmatic school, Moore for instance, resisted the new ideas.[45] Parkes was not among them. As early as his annual report of 1870, long before a Pasteur or a Koch had made any significant discoveries, he noted with approval interesting work being done on "low forms of animal and plant life." According to a primitive form of the germ theory, "The agents of contagion are considered to be minute particles of bioplasm, which can be seen under the microscope in vaccine lymph, variolous

[41] *Ibid.*
[42] *PP*, 1870 [C. 138], vol. 43, Army Med. Dept., AR (1868).
[43] *PP*, 1873 [C. 787], vol. 41, Army Med. Dept., AR (1871).
[44] *PP*, 1870 [C. 138], vol. 43, Army Med. Dept., AR (1868).
[45] The 1894 edition of Moore's *Family Medicine in India* contains virtually no reference to the germ theory of disease.

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pus, blood serum of cattle-plague, etc."[46] So far no disease had been associated with any particular variety of bioplasm. There was need as well as room for skepticism. Dr. Douglas Cunningham, one of the cholera investigators, came under attack in the mid-1870s for not following the latest European theory. "It is the doctrine of contagion," he complained, "which has prevented progress. So long as men believe that all that is required to escape attack is to shun the sick, what interest can there be in sanitary reforms?"[47] Commented the Anglo-Indian newspaper *The Pioneer* in his defense: "The germs of epidemic disease have never been seen, diligent search has altogether failed to find them, and a thousand facts, especially in this country, are opposed to their existence. In fact the whole germ theory, as our knowledge now stands, rests purely on imagination."[48] Moreover, unless the medical authorities could prove that a disease was contagious, the public would justifiably resist a quarantine or other interference, even granting the dubious assumption that such measures were effective. It was far more practical to take the common-sense view, which associated disease with filth, and act accordingly.

Like so many scientific discoveries, the germ theory did rest initially on imagination, which suggested to researchers where they ought to look first. By the early 1890s, however, the theory was clearly no longer mere speculation. In 1883 Koch identified the so-called comma bacillus in water tanks in parts of Calcutta that were heavily infected by cholera, and the bacillus of typhoid fever was also isolated. By 1895 the Government of India had employed its own microbiologists, who were available for routine inspections as well as for emergencies. At first, however, the results were disappointing. When microbiologists investigated an epidemic of typhoid near the Calcutta cantonment of Barrackpore, for instance, they found the bacillus in milk, water, food, feces, flies—literally everywhere. A disillusioned medical officer wondered just how much they had learned. "That the habits of the class of natives employed in kitchens are filthy, must be admitted, that the enteric microbe appears to be widely disseminated in India is of course known." Therefore, he was not surprised to be informed that the bacillus had shown up in kitchens.[49] But what was he supposed to do next? Unless the complete etiology of the disease were known, to be told that germs were everywhere was not much of a practical advance on the miasmatic approach.[50]

The annual sanitary report of 1894–95 concluded that sanitary workers must not be content with following the latest expert information, for the microbiologists would probably solve only part of the problem. The only practical policy was to cover all possibilities. "It is not enough that air, earth, water, and men should be kept scrupulously clean in cantonments. . . . It is necessary to extend the area of cleanliness far beyond cantonment limits, in fact, ultimately throughout the length

[46] *PP*, 1872 [C. 604], vol. 38.
[47] *PP*, 1876 [C. 1615], vol. 56, Annual Report on Sanitary Measures, East India (1874–75).
[48] *The Pioneer*, January 17, 1876, in *ibid.*
[49] *PP*, 1898 [C. 8936], vol. 54, Army Med. Dept., AR (1897).
[50] Even though the germ theory has long been unquestioned, the practical traveler still operates on theories of miasma. Dysentery, for instance, is always contracted from a single source, infected by a previously infected person. But the disease is so prevalent that it is "in the environment," and the traveler should act accordingly.

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and breadth of India." British troops "will never be safe so long as the native population and its towns and villages are left uncleansed to act as a reservoir of dirt and disease." In large cities, where Europeans lived in sizeable numbers and where educated Indians had sometimes cooperated, progress had been made, notably in improving water supplies and systems of conservancy. India's one hundred thousand villages, however, remained untouched. The task of cleaning up India was daunting, but it had to be faced. For "the present system of cleansing small areas for the European troops," which though never an articulated policy had been adopted by default, "and leaving the surroundings of these sanitary cases as filthy as ever, has proved to be neither very cheap, nor sufficiently effectual."[51]

The Government of India's declaration that its medical and sanitary services were not restricted to Europeans soon received a severe test. In 1896 the fourth (some say the third) pandemic of bubonic plague, which had spread from the Yunan Province of south China, reaching Canton and Hong Kong in 1894, struck Bombay.[52] Once the plague bacillus had been identified, the military authorities declared Bombay City off-limits, a measure that (in addition to the greater infestation of Indian houses and barracks by rats) succeeded in protecting British troops and Europeans in general from more than a minuscule number of cases. Whatever their faults, and they were many, the Government's medical officers responded vigorously if futilely.

Neither the association of *Bacterium pestis*[53] with plague nor its presence in the infected area was seriously questioned. But isolating the plague bacteria was only the beginning of a difficult problem not satisfactorily resolved until 1914. Was the bacillus the only cause? If so, how did it enter the human system? How was it spread? From person to person? Oddly, doctors and nurses, who treated plague patients, and sweepers, who trod barefoot from morning to night over human excrement, including that of plague victims, did not seem greatly at risk. The prevalence of the disease in dock areas and among grain merchants suggested that it had come from China (though a northeastern Indian origin was also possible) and that it was in some way related to rats, whose corpses had been extremely plentiful shortly before the outbreak. But even if the rat hypothesis were true—it had been suggested for centuries and, particularly after its confirmation, was discovered to have been embedded in numerous folk ideas about the disease, including the legend of the Pied Piper of Hamelin—how were rats infected, and how did the affliction spread from them to man? Did rats and man contract it from

[51] *PP*, 1896 [C. 8237], vol. 62, Sanitary Dept., AR (1894–95).

[52] For a general study of the current pandemic of bubonic plague, see William H. McNeill, *Plagues and Peoples* (Garden City, N.Y., 1976).

[53] After 1900 the name for plague bacteria was changed to *Bacillus pestis* and, in 1923, to *Pasteurella pestis*. In the late 1960s microbiologists changed the name again to *Yersinia pestis*, after Alexander Yersin, the principal discoverer, and linked plague to nonplague bacteria, which are also transferred through the bloodstream and often infect the lymph nodes, causing comparatively rare acute intestinal disorders. The standard medical textbook, which contains an excellent historical section, is Thomas Butler's *Plague and other Yersinia Infections* (New York, 1983). Also see L. Fabian Hirst, *The Conquest of Plague: A Study of the Evolution of Epidemiology* (Oxford, 1953).

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the same source? In short, despite the discovery of the bacterial agent, the old question remained. Was plague contagious or miasmatic?

Europeans reacted to the epidemic with dread and fascination. Plague commissions—Austrian and German as well as British (the Indian Plague Commission)—set up shop. Understandably, the Bombay medical authorities acted on both the contagion and miasma hypotheses. They quarantined suspicious ships; they conducted rigorous house-to-house searches for victims, whom they took to plague hospitals; they segregated suspected carriers and screened railway travelers. Just as vigorously they disinfected sewers, gutters, and houses, whitewashing and spraying with abandon, often tearing off roofs. Naturally the atmosphere was tense. Rumors spread that the Government was carting off people to plague hospitals in order to kill them. Hindus and especially Muslims deeply resented violations of purdah. Worst of all, the anti-plague measures did not work. Annually, in Bombay City, the disease repeated what became its familiar cycle—a few cases in the early autumn, a gradual increase in number, then a climax in January-March, before a decline and eventual end in the hot weather. At first the authorities took heart that their measures had succeeded but were disillusioned by late September or October. And plague spread rapidly—to Poona and throughout Bombay Presidency, to Karachi, to Bangalore, to the Punjab—but for some unknown reason not to epidemic proportions in Calcutta.[54]

Deeply distrustful of the British government and medical authorities, Indians reacted sharply. In large numbers—perhaps more than half of the population of Bombay—they fled, so that cotton mills had to close down. They resisted passively, in many cases refusing to notify the authorities of victims or deaths. In Bombay and Poona they rioted. The Extremist leader B. G. Tilak made the anti-plague campaign a central issue in nationalist agitation. In Poona a health officer was assassinated. At length the government concluded that, especially in cities, without the cooperation of the people, draconian public health measures would not only be unsuccessful but would probably cause more harm than good.[55] The tactics changed. Patients were allowed to remain at home; women were no longer dragged into the streets; the authorities largely gave up trying to quarantine infected areas. The numbers of cases, the medical officers had to concede, apparently neither rose nor fell. The stark truth was that the people of India might as well have been living in the fourteenth century.

The ordinary annual reports of the sanitation department turn out to have been a good deal closer to the solution of the problem than those of the supposedly expert Indian Plague Commission, which gathered immense quantities of information and examined numerous witnesses on the spot. For, though the plague commission did pay some attention to the rat hypothesis, its report stressed that the theory was unproven (which, of course, until the flea vector was finally confirmed, was true), even though many authorities—including Koch and

[54] *PP*, 1900 [Cd. 139–41], vol. 30–32, and 1902 [Cd. 809–10], vol. 53, Indian Plague Commission.

[55] *Ibid.* The only tactic that did seem to work became routine in Punjab villages, which district officers could more effectively control. With the first plague cases, and later with the discovery of dead rats, the villagers would leave and live in the open fields.

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Manson—accepted it at once.[56] Rats, the commissioners stressed, were only one possible means of infection and probably not the most important. "Plague," they quoted Robert Hervey, the director of the Indian Medical Service, "is a disease propagated by a specific germ, which springs from a preexisting germ, and cannot exist de novo." The "plague seed cannot flourish unless it finds an environment suitable to it, and that environment is made by all these insanitary conditions. It is a disease of filth, a disease of dirt, and a disease of poverty."[57] The key point was the hypothesis, proposed but left unproven by P.-L. Simond in 1898, that rat fleas escape from their dead normal hosts, transferring the infection to humans by biting them. The sanitation department reported the theory fully, suggesting that it was worthy of further research. Clearly misstating the conclusion of an important review article on the whole subject of the role of suctorial insects in spreading diseases, the Indian Plague Commission dismissed Simond with scornful sarcasm.[58] Its favorite theory was that barefoot Indians were infected from bacilli lurking in cow-dung floors. So, despite the disheartening information that a square foot of cow dung contains millions of bacteria, and will unless literally transformed into a solution of hydrochloric acid, the Bombay authorities tried to disinfect the floors, too.[59]

In retrospect, it is unrealistic to suppose that the Government of India could have done anything to prevent plague as an endemic disease in the subcontinent, where it caused some thirteen million deaths before mysteriously vanishing in the 1960s. (The idea that governments ought to be responsible for the health of their citizens is a recent innovation.) Vaccines were both unreliable and dangerous. In one case, a contaminated dose killed off an entire Punjab village. Antibiotic drugs, available only since World War II, have alone proved effective. The Government's assumption that plague was a filth disease was not wholly incorrect, of course, since rats are associated with filth. But the strategy of disinfection, driving rats out of their nests and especially from sewers, probably did more harm than good. Indeed, according to the latest guidelines of the World Health Organization, the commonsense strategy of killing rats—which the Government adopted once the rat-flea

[56] The Indian Plague Commission was sharply attacked for its relative inattention to the rat hypothesis. Koch called "plague primarily and essentially a rat's disease; from rats the infection is carried to man"; *BMJ*, July 16, 1898. Manson agreed: "It seems to me that the present measures of plague prevention are like bolting the window against a thief whilst the front door is left wide open; moreover, they are exceedingly expensive, exceedingly irksome, exceedingly unpopuar, and exceedingly inefficient." The obvious solution, he concluded (as it turns out, probably mistakenly), was to direct all efforts toward killing rats; *BMJ*, October 7, 1899.

[57] *PP*, 1900 [Cd. 139], vol. 30, Indian Plague Commission.

[58] *Ibid.* For a key article, see George H. Nuttall, "On the Role of Insects, Arachnids, and Myriapods, as Carriers in the Spread of Bacterial and Parasitic Diseases of Man and Animals: A Critical and Historical Study," *Johns Hopkins Hospital Reports*, 8 (1900): 1–154. Also see P.-L. Simond, "La Propagation de la peste," *Annales de L'Institut Pasteur*, 12 (1898): 635–87.

[59] The barefoot theory was soon discredited by an epidemic of plague in Sydney, where people no more went barefoot than they did in London, and medical opinion continued to swing over to the flea hypothesis. See J. Ashburton Thompson, "A Contribution to the Aetiology of Plague," *Journal of Hygiene*, 1 (1901): 153–67. When the Plague Research Commission (not to be confused with the Indian Plague Commission) embarked in 1904 on the research that eventually solved the problem, it began with the task of confirming or refuting the rat-flea hypothesis. Plague Research Committee, Special Plague Number, *Journal of Hygiene*, 6 (1906), 423; and *PP*, 1906 [Cd. 3152], vol. 82, Sanitary Dept., AR (1904–05).

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hypothesis was widely accepted—is not advisable either. For the rats will come back, soon matching or surpassing their former numbers, and in the short run the proper target is not rats but fleas. Only in very limited areas, such as airports, are extermination campaigns recommended, and then it is extremely important to get the sequence right—first insecticides against fleas, then poison against rats. Otherwise, killing rats drives fleas to man.[60] It appears that the most effective anti-plague measures were taken by the rats themselves; recent surveys have found extremely high percentages of Bombay rats to be resistant.[61]

Compared with plague, a sensational and unfamiliar disease, more common afflictions—especially cholera and malaria—continued to be much more destructive. The average number of deaths from malaria alone was estimated at one million a year, the number of nonfatal but debilitating cases at one hundred to one or higher.[62] Although the sanitation department's annual reports had given malaria surprisingly little attention, in the mid-1890s, with the publication of Manson's ingenious (though flawed) mosquito hypothesis, the British Indian medical authorities began to follow the progress of research closely.[63] Once Ross discovered in 1897 the sexual cycle of female *Anopheles* mosquitoes, the crux of the problem was resolved, and others (notably Italians) soon filled in the other essential links. The conversion of the medical profession was rapid. By 1900 searches for *Anopheles* mosquitoes and their breeding grounds, microscopic analyses of blood samples, counts of enlarged spleens, especially among Indian children (a rough guide to the degree of endemicity in a particular area), and the sale of cheap quinine packets in post offices had all become routine. Reviewing the impressive progress of medical research in the past two decades, which indeed had encompassed the rise and establishment of the germ theory of disease, the annual sanitation report at the turn of the century looked forward to a "future bright with promise. The idea of banishing malarial fevers entirely from the list of ills in this country is magnificent, and it is impossible at this stage to forecast how nearly that consummation might be achieved if the interest and intelligence of the people could be enlisted in this campaign."[64] The report warned, however, that the obstacles were far more numerous than, say, in Italy, where the state was conducting a well-publicized campaign, or in Panama, where the American army could do as it pleased. Most of the effect in India had to be concentrated on cantonments and prisons, small populations where the state could exercise complete control, using quinine parades, for example, to insure that every person took the proper prophylactic doses on schedule.

[60] See M. Bahmanyar and D. C. Cavanaugh, *Plague Manual* (Geneva, 1976).

[61] See Butler, *Plague and other Yersinia Infections*, 49, 54. It seems likely that the ability of rats to breed immunity may have been responsible for the decline of plague in Europe in the fifteenth century and in England in the seventeenth.

[62] *PP*, 1912–13 [Cd. 6538], vol. 62, Imperial Malaria Conference, Simla, October 1909. A great problem was that doctors, let alone village registrars, had difficulty distinguishing between malaria and other kinds of "fever."

[63] *PP*, 1899 [C. 9549], vol. 66, Annual Sanitary Report, 1897–98, pt. 2.

[64] *PP*, 1902 [Cd. 1357], vol. 74, Annual Sanitary Report, 1900–01.

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With the discovery of the essential etiological facts of the disease, the plane of the malaria debate shifted from questions of causation to those of treatment and especially of prevention. Once it was agreed that malaria is caused by a parasite whose life cycle depends on access to two hosts (*Anopheles* mosquitoes and human beings), then two strategies, or perhaps a combination of those strategies, could be followed. Ross established mosquito brigades, which concentrated on eradication by means of drainage, systematic sanitary inspections, and killing the larvae with kerosene and the adults with insecticides.[65] The alternative, pioneered by Koch in New Guinea and favored by the Italians in the Roman Campagna, was the mass distribution of quinine to residents of malarious areas. Both methods had disadvantages. Given the quality of insecticides and the means of spraying them that were then available, the anti-mosquito approach was effective only in urban areas and even then required continuous effort, for, like rats and fleas, mosquitoes had an ecological niche from which it was impossible to dislodge them. In rural areas the potential breeding grounds for mosquitoes during monsoon seasons approached infinity.[66] The disadvantages of administering quinine were cost (assuming that every potential victim in a country like India had to receive regular doses, so many times a week) and effective distribution to a largely illiterate population, hampered further by poor systems of communications.[67]

Within a very short time after the confirmation of the basic facts about malaria, the debate was joined between the protagonists of these two strategies, in India and elsewhere. Despite vast improvements in medical knowledge and technology, despite the accumulation of considerable experience with the problems of eradication, the discussion has proceeded along essentially the same lines throughout the twentieth century. In practice, both sides ordinarily conceded that the two approaches should be combined, depending on local circumstances. Yet the debate was controversial and passionate. Too much attention to quinine, argued Ross and others, would mean neglect of drainage and sanitary projects. But eradication of the mosquito, rejoined the quinine advocates, while perhaps advisable in limited and especially in urban areas, was pursuing a chimera—the mosquitoes would always win. The inflated rhetoric encouraged both sides to adopt a totalitarian goal—the utter elimination from India and perhaps elsewhere of malaria, which was unrealistic considering the prevailing state of medical knowledge and technology.

[65] See Ronald Ross, *Mosquito Brigades and How to Organize Them* (London, 1902). Also see Ross *et al.*, *The Prevention of Malaria* (London, 1910).

[66] By "exterminate" Ross really meant reducing the number of mosquitoes in a given area to a level that would have had a statistical impact on the propagation and survival of the disease. See Ronald Ross, *Memoirs: With a Full Account of the Great Malaria Problem and its Solution* (London, 1923).

[67] Here again, the proponents argued that a blanket campaign—every Indian or every Bengali taking the prescribed prophylactic doses throughout the malarial season—would not be necessary. Less than complete coverage would still reduce substantially the chances of the parasite's survival. Fortunately, not all parts of India were equally malarious; only the high-incidence districts, or even parts of districts, needed to be targeted. Paper by Major S. P. James, Imperial Malaria Conference (1909), *PP*, 1912–13 [Cd. 6538], vol. 62.

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IT IS IN THE CONTEXT OF THIS GENERAL DEBATE on malaria, with its emphasis on attacking the disease in the Indian population at large, that the advocacy of segregation by the Royal Society's expedition to West Africa must be seen. But the Royal Society's experts addressed an essentially different debate, which proceeded from different assumptions and which had a different agenda: the use of tropical medicine, with its newly founded schools in London and Liverpool, to open up and exploit what Joseph Chamberlain called the British empire's undeveloped estates. The fundamental assumption of the second debate was that the single most important factor for the development of tropical colonies was enabling substantial numbers of Europeans to live and work in them.[68] It was these much narrower possibilities, not the conquest of malaria in general, that Drs. J. W. Stephens and S. R. Christophers (later joined by C. W. Daniels) were sent to West Africa to investigate.

The Royal Society's team did inject some badly needed realism.[69] The obstacles, they concluded correctly, were enormous. By examining spleens they confirmed Koch's finding that, while adult Africans developed some degree of immunity, endemicity among children was practically universal. During wet seasons, the numbers and breeding places of *Anopheles*, as well as of other species that carry yellow fever, were so voluminous that the idea of eliminating mosquitoes had to be rejected as impracticable. And it was doubtful that using quinine as a prophylaxis would succeed, except among a small elite of educated Africans in a town such as Lagos. Only a small minority, again, would be able to afford expensive screens or nets.

In fact, Stephens and Christophers concluded, the only practicable strategy for Europeans in West Africa was to isolate themselves from infected Africans—and all Africans must be assumed to be infected. In Freetown, Sierra Leone, they found, *Anopheles* tended to congregate in the dark, dirty huts of Africans. Plans were afoot, they reported, to build a light railway to a new European settlement on nearby hills. Although that was all to the good, it would accomplish little, they warned, if the sleeping quarters of Africans were not removed as far as possible from the settlement.[70]

In the Gold Coast, Stephens and Christophers found additional evidence for both their pessimism and their favorite remedy. Indeed, they developed an analogy between malaria and ngana, a cattle disease transmitted by the tsetse fly from wild animals to domestic. It had long been noted, they pointed out, that the destruction of buffalo and wild game in a district eliminated both the tsetse fly and ngana. "In the very remarkable general infection of native children with an apparently mild form of malaria, and the close relation of anopheles to native

[68] On the role of tropical medicine in Chamberlain's undeveloped estates campaign, see Robert V. Kubicek, *The Administration of Imperialism: Joseph Chamberlain at the Colonial Office* (Durham, N.C., 1969), chap. 7; and Dumett, "Campaign against Malaria."

[69] The World Health Organization has consistently excluded Africa from its anti-malaria campaigns, choosing to concentrate first on Asia and South America; then, assuming success in those regions, would come Africa. See Harrison, *Mosquitoes, Malaria, and Man*.

[70] J. W. Stephens and S. R. Christophers, "Distribution of Anopheles in Sierra Leone," in Royal Society of London, *Reports to the Malaria Committee* (London, 1900–03).

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dwellings . . . , and in the transmission thereby of malaria to Europeans, there is a process resembling that which takes place in ngana." Moreover, "the analogy holds good in the conversion of the mild or innocuous type of the native disease into the virulent form in Europeans." The implied strategy was the elimination, at least from proximity to the sleeping quarters of Europeans, of such wild species as African children.[71]

In both Sierra Leone and the Gold Coast, Stephens and Christophers found already in existence embryonic plans for separate European areas in (or near) African cities, which they encouraged on grounds of medical science.[72] Further south, in Lagos, they encountered Governor William MacGregor, who was a passionate advocate of another school. MacGregor corresponded with Ross, undertook a visit to the headquarters of the Italian anti-malaria campaign in the Roman Campagna, equipped Government House with screens and nets as an example, and sought the support of the educated African community in a brave attempt to eliminate the disease. The medical service, he maintained, was there primarily to treat Africans, whose increased productivity (once freed of malaria) would be the base from which the improvement of undeveloped estates would rise.[73] Sketchily and without enthusiasm, Stephens and Christophers reported on drainage and attempts to destroy breeding grounds. Their investigations revealed what they considered to be only temporary success. For the time being *Anopheles* were indeed comparatively rare, and they found a slight reduction in the percentage of enlarged spleens among African children. But those apparently encouraging results had been obtained in the dry season. Once the rains came the mosquitoes would return, and the endemicity would no doubt rise to the expected level. "In Africa," they concluded, over MacGregor's strenuous objections, "the primary aim should not be the destruction of anopheles, but rather to remove susceptible Europeans from the midst of malaria. To stamp out native malaria is at present chimerical, and every effort should rather be turned to the protection of Europeans."[74]

In their concluding report on Africa, Stephens and Christophers spelled out once more the arguments for their solution, which they now called segregation. Referring to Koch's research among children in New Guinea, they found a similarly high degree of endemicity in West Africa. They repeated the analogy with animal diseases. "Just as a horse cannot live in a 'fly' district, as he becomes inoculated with the fatal tsetse-fly disease by the contagion conveyed from the wild game, so the European living among native children is inoculated by means of the mosquito with malaria. The only means of escape is by segregation to avoid the source of contagion." Such general campaigns as MacGregor's must be written off

[71] J. W. Stephens and S. R. Christophers, "The Native as the Prime Agent in the Malarial Infection of Europeans," *ibid.*

[72] See Christopher Fyfe, *A History of Sierra Leone* (London, 1962), 610–11: "Thus the Indian model of a racially stratified society was introduced into Sierra Leone, not shamefacedly as required by prejudice, but openly as dictated by medical science."

[73] MacGregor, "Notes on Antimalarial Measures in Lagos," *BMJ*, September 14, 1901.

[74] J. W. Stephens and S. R. Christophers, "On the Destruction of Anopheles in Lagos," in Royal Society of London, *Reports to the Malaria Committee*.

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as futile, as unwise and unwarranted expenditures of scarce public funds. Perhaps the long-term decline of malaria would result from the gradual extension of cultivation, and therefore of drainage, and from general improvements in sanitation. Meanwhile, they concluded, in a classic racist statement, Europeans would find in segregation a simple and fully effective mode of prevention: "In Africa a complete isolation of Europeans would, we believe, render malaria a comparatively rare disease."[75]

In Africa, as Curtin explained, the Royal Society team's reports provided a scientific rationale for the development of town plans based on patterns of racial stratification and segregation.[76] Contemporary reports from expeditions dispatched by the Liverpool School of Tropical Medicine, led by Ross himself, were written in a different vein, mentioning the isolation of European sleeping quarters only briefly and well down on the list of several recommendations, the most important of which were drainage and sanitation. Stephens and Christophers, who after all were comparative nobodies, might abandon the general goal of eradicating malaria. The far better known Ross, who never forgave the Indian and colonial authorities for not appointing him the tsar of an empire-wide assault on the disease, did not.[77]

Evidently the popularity of segregation as the remedy for malaria among Europeans in West Africa depended far less on the prestige of those who proposed it than on the motives of the colonial authorities who adopted it. Lugard's motivation, at least, is not particularly mysterious. He was attempting to articulate and justify a system of control, a structure of power in a colonial regime, which would be run as cheaply as possible "along African lines." For about a decade the segregationist fad continued, before the strong criticisms of educated Africans, who argued that the goal ought to be eradicating the disease among the people who lived there, persuaded governors like Sir Hugh Clifford (if not Lugard) to abandon the rhetoric—though not the practice. By then the towns had been built. The word "segregation," ordinarily used in nineteenth-century medical literature with reference to sick individuals and their contacts but employed by Stephens and Christophers in 1900 as a class term, soon became, in South Africa, America, and elsewhere, a key word in the vocabulary of race relations in the twentieth century.[78]

From Africa the inventive Stephens and Christophers proceeded to India, where they were joined by Captain S. P. James of the Indian Medical Service in a systematic investigation of malaria, which required several years at the notoriously unhealthy cantonment of Mian Mir, in the Bari Doab canal area of the Punjab. Although Ross was deeply offended by the Mian Mir experiment—he believed he had provided enough information for any government to form a

[75] J. W. Stephens and S. R. Christophers, as quoted by Curtin, "Medical Knowledge and Urban Planning," 599.

[76] Curtin, *ibid.*

[77] Ross, *Memoirs*, 317: "I hope the people of India will forgive these people some day—certainly I shall never do so." Also see Ronald Ross *et al.*, *Report of the Malaria Expedition*, Liverpool School of Tropical Medicine Memoir No. 2 (Liverpool, 1900), and *Instructions for the Prevention of Malarial Fever, for the Use of Residents in Malarial Places* (2d edn., Liverpool, 1900).

[78] See my *Highest Stage of White Supremacy*, esp. chap. 1.

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policy[79]—the work there was an intensive and extremely thorough piece of research, a landmark in the history of malaria in India.

Once again, the team cast doubt on what its reports called wildly optimistic schemes. After preliminary research in Calcutta, where the investigators found plenty of *Anopheles* mosquitoes but surprisingly little malaria, they settled on Mian Mir, which they selected because it was a comparatively small area known to be highly malarious and because they could be sure that both the military and civilian population would cooperate. Although further research had shown that factors other than malaria can cause enlarged spleens, which showed up for example in a number of children in London's East End,[80] Christophers continued to measure the spleens of the children of Indian soldiers and followers in order to arrive at a rough index of endemicity.[81] Predictably it was high. Painstakingly the team compiled statistics on malaria, verified by microscopic examination of blood samples among various groups: European soldiers (officers and enlisted men in the infantry, artillery, and so forth), Indian soldiers, European and Indian military families, followers and their families, and residents of nearby villages. Using some groups as controls, they tested varying dosages of quinine to determine what would be both safe and sufficient as a prophylaxis. Indian children, they reported, would readily take quinine, particularly if mixed in sweets.

Finally, well aware that Ross and his supporters would scrutinize their reports, they went after the mosquitoes.[82] The first year they searched out adults and the breeding grounds for larvae, treating the latter with kerosene. During the dry season they thought they might have made some headway. But the monsoons revealed much the same numbers of *Anopheles* as before; either they had flown in or they had bred in the innumerable small puddles. Recognizing that one year would not suffice for a fair test, the team repeated the operations the next, with precisely the same results. For the resourceful *Anopheles* could deposit thousands of larvae in the tiny puddle left by the footprint of a single cow. Even worse, they could breed in the running water of irrigation ditches. There it was: the precious irrigation water, on which the livelihood of the prosperous and growing Punjab canal colonies depended, could not be made unfit for mosquitoes without also destroying its use for crops, animals, and man.[83]

What is interesting, in the light of the explicitly racist rhetoric of the earlier West African reports, is the subdued tone of those from Mian Mir. Perhaps the difference is that James wrote most of them, for, at least when they were writing for West African consumption, Stephens and Christophers remained unapolo-

[79] Ross, *Memoirs*, 314–15.

[80] *PP*, 1906 [Cd. 3212], vol. 68, Army Med. Dept., AR (1905).

[81] For Christophers's later contribution, see Sir Richard Christophers, "Endemic and Epidemic Prevalence," in Mark Boyd, ed., *Malariology: A Comprehensive Survey of all Aspects of this Group from a Global Standpoint* (Philadelphia, 1949), 698–721. This article mentions the reports by Stephens and Christophers in 1900–01 but not, of course, their early advocacy of segregation.

[82] For Ross's testy response to the Mian Mir report, in the general discussion of the prophylaxis of malaria, see *BMJ*, September 17, 1904.

[83] *PP*, 1902 [Cd. 1357], vol. 74; 1904 [Cd. 1850], vol. 67; 1905 [Cd. 2298], vol. 58; 1906 [Cd. 2776], vol. 82, Annual Sanitary Reports, 1900–01 through 1903–04.

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getic.[84] Perhaps they were warned to guard their language in documents that were subject to scrutiny and used as the basis for attack by alert Indian journalists or by G. K. Gokhale, the so-called Moderate leader, in his annual critical comments in the Legislative Council on budget day.[85] Or perhaps the difference in tone is the result of a difference in context—Curzon's India, not Chamberlain's undeveloped tropical estates. Segregation is not among the major recommendations of the Mian Mir reports. The closest thing to it is a brief statement that, since higher malarial rates had been discovered in barracks closest to the followers' lines, it might be as well to move those lines back a bit, but the language was so innocuous that the most sensitive Indian could not have objected.

Despite the writings of Lugard,[86] in West Africa the fad of justifying segregated living patterns on grounds of protection against malaria was comparatively short-lived. In India it never really began. In practice the Government of India continued to give the troops and other Europeans preferred medical treatment, in part because their smaller numbers made it possible. In Indian town plans, which came into fashion about the same time that they did in Africa, segregated and racially stratified patterns were developed—civil lines, gymkhana clubs, race and golf courses, and the rest—but not with elaborate medical rationales.

Indeed, the Government of India's policy on malaria was set out in annual malaria conferences.[87] The stated goal stayed the same. Within the limits imposed by the state of medical science and technology, by how much an alien government could intervene in the lives of Indian subjects, and by financial resources, the Government declared its determination to try to eradicate the disease. Those limits were severe. Not until World War II did effective insecticides, notably DDT, become available. After independence, in concert with the World Health Organization, the Government launched the kind of anti-mosquito campaign that came close to fulfilling Ross's dreams, for it succeeded by the early 1970s in bringing

[84] "Since we first put forward segregation as a principle to be followed whenever opportunity offered," the team at Mian Mir wrote in 1903, "it has been recognised by some authorities [including the great Manson] as the first law of hygiene in the tropics." Unfortunately, they had received some criticism, but that had been based on a misunderstanding. "Segregation as an anti-malarial measure does not . . . mean the avoidance of intercourse with the native. Nor does it mean a lessening of the power of control of the native. . . . In India segregation is almost universal, . . . yet . . . there can be no question of loss of touch with the natives—rather on the contrary, an increased respect on their part." *Summary of Researches on Native Malaria and Malarial Prophylaxis: On Blackwater Fever, Its Nature and Prophylaxis* (London, 1903), as quoted by Kubicek, *Administration of Imperialism*, 147–48.

[85] See B. R. Nanda, *Gokhale: The Indian Moderates and the British Raj* (Princeton, N.J., 1977); and Stanley A. Wolpert, *Tilak and Gokhale: Revolution and Reform in the Making of Modern India* (Berkeley and Los Angeles, 1962).

[86] Frederick D. Lugard, *The Dual Mandate in British Tropical Africa* (1922; reprint edn., London, 1965), 148–50: "We have learnt that malarial germs . . . are present in the blood of most natives, especially of native children, and their dark huts and insanitary surroundings foster mosquitoes, by which these diseases are conveyed. Doctors, therefore, urge that Europeans should not sleep in proximity to natives, in order to avoid infection." Then followed detailed directions for laying out European reserves, virtually unchanged from the *Political Memoranda*. Alas, Lugard conceded, segregation had been somewhat controversial, "in part due to a misapprehension, in part to an attempt to carry the policy to extremes," though who might have been a greater extremist than himself is not entirely clear. But, since educated Indian and African "gentlemen" had allowed themselves to become rather upset over the issue, it should "be made abundantly clear that what is aimed at is a segregation of social standards, and not a segregation of races."

[87] *PP*, 1912–13 [Cd. 6538], vol. 62, Imperial Malaria Conference.

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down the number of cases to only a few thousand per year. But the *Anopheles* mosquito remained ever resourceful, breeding resistance to DDT, which had such adverse environmental effects that it had to be abandoned. By the end of the decade the number of cases was back up to 30 million. In certain areas the malaria parasite, equally resilient, bred resistance to chloroquinine, the widespread quinine substitute.[88] In retrospect the Indian Medical Service's optimism that the elimination of malaria from the subcontinent was within its grasp was unjustified. The service understated the severity of the problems and overstated both its resources and, not least, the strength of its own will. Better undue optimism, however, than a deliberate and explicitly segregationist policy aimed only at protecting Europeans from the Indians among whom they served.

[88] Harrison, *Mosquitoes, Malaria, and Man.*

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Psychohistory as History

THOMAS A. KOHUT

IN ITS BRIEF AND NOT ALTOGETHER HAPPY existence, psychohistory has aroused considerable controversy. "Traditional" political and intellectual historians, suspicious of recent efforts to apply the theories and methods of other disciplines to the past, have criticized psychohistorians for emphasizing unconscious motive rather than conscious purpose.[1] "Modern" social historians, irritated with traditional political and intellectual history, have criticized psychohistorians for focusing on the individual and failing to account for the social determinants of psychological attitudes and individual actions.[2] Those engaged in debate over historical epistemology have argued that the approach psychohistorians have taken to the past lacks logical rigor.[3] But, as Fred Weinstein has pointed out, some of the critiques of psychohistorical methodology can be applied by extension to traditional historical methodology. Several of David E. Stannard's arguments in *Shrinking History*, for example, call into question not only psychohistory and psychoanalysis but also the practice of history itself.[4] Uneasy about the methodology of their discipline, historians of differing persuasions nonetheless presume

A number of people have contributed their criticism and advice to this article, not all of whom can be mentioned here. I am particularly grateful to John P. Demos, Peter Loewenberg, and Charles B. Strozier. Among my friends and colleagues at Williams College who read drafts of this essay, I would like to acknowledge Robert G. L. Waite, James B. Wood, and, above all, William G. Wagner. This article owes most, however, to two of my teachers, one a historian and the other a psychoanalyst: Otto P. Pflanze and John A. MacLeod.

[1] See, for example, Jacques Barzun, *Clio and the Doctors: Psycho-History, Quanto-History, and History* (Chicago, 1974); and Gerald Izenberg, "Psychohistory and Intellectual History," *History and Theory*, 14 (1975): 139–55. The critique of psychohistorians' emphasis on motive rather than on conscious purpose is also applicable to those social historians who see people acting from socioeconomic motives of which they are largely unaware.

[2] See, for example, Hans-Ulrich Wehler, "Psychoanalysis and History," *Social Research*, 47 (1980): 519–36.

[3] See, for example, David Hackett Fischer, *Historians' Fallacies: Toward a Logic of Historical Thought* (New York, 1970); and David E. Stannard, *Shrinking History: On Freud and the Failure of Psychohistory* (New York, 1980).

[4] Stannard attacked psychoanalysis from an essentially positivist perspective, seemingly unaware that history with its inherent subjectivity and reliance on empathic understanding is, as this article seeks to demonstrate, no more solidly empirical than is the clinical practice of psychoanalysis.

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that psychohistory can be criticized with impunity. Psychohistory, it seems, has become a lightning rod, able to absorb some of the historical profession's uncertainty about itself.

While embroiled in controversies that transcend their field of inquiry, psychohistorians have also been troubled by serious problems of their own making. Too many studies are superficial and even irresponsible, falling well below the level of scholarship traditionally demanded of the historical profession. Some studies—even by thoughtful and responsible investigators—are reductionistic and, when they fail to account sufficiently for differences in time and place, unhistorical.[5]

The deficiencies of these works do not derive inevitably from the psychological nature of the subject being investigated, as a number of critics of psychohistory have assumed, but are the result of the manner in which the psychohistorians have conducted their investigations. In some cases, deficiencies can be attributed to the vagaries of the authors. For the most part, however, they stem from the method used in these studies. For several reasons, many psychohistorians have assumed that, in order to know the psychological in history, it is at times necessary to supplement or even to supplant the traditional historical approach with a psychohistorical method. Although the difference between this method and the traditional historical approach is more a matter of degree than of kind, the application of the psychohistorical method to the past is problematical. It is also unnecessary.

The bridge from history to psychohistory is easier to cross than some critics of psychohistory and some psychohistorians assume. Because the historian and the psychoanalyst approach their human subjects in essentially the same way and share a common method of understanding and explanation, the psychological dimension of the past can be known using methods traditionally employed by historians, and psychohistory can be written as history. The application of traditional historical methods coupled with a sensitivity to human psychology can lead, in fact has led, to historical studies that measure up to the standards of the profession—some by historians who have no consciousness of having crossed the bridge into psychohistory. Therefore, although the criticism of certain psychohistorical studies and the psychohistorical method used in them is justified, the conclusion of some critics that the effort to understand the psychological dimension of the past should be abandoned is not. That dimension is a legitimate subject of historical inquiry, indeed one of the most important aspects of man's past that the historian can investigate.

THE PSYCHOHISTORICAL METHOD has been variously defined and dissected by both the supporters and the detractors of psychohistory. Nevertheless, the method can be seen as having two essential characteristics that distinguish it from traditional approaches to the past. In the first place, the psychohistorical method relies on

[5] In addition to the works already cited, see Robert Coles, "Shrinking History," *New York Review of Books*, February 23, March 8, 1973, pp. 15–21, 25–29; and Kenneth Lynn, "History's Reckless Psychologizing," *Chronicle of Higher Education*, January 16, 1978, p. 48.

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theory, particularly psychoanalytic theory, to provide historical understanding and explanation. Figures and events from the past are not comprehended or made comprehensible on their own terms but are understood and explained primarily by psychological theory. Too often, when employing the psychohistorical method, the historian comes to the past with an understanding and explanation already in hand; the understanding and explanation do not emerge from the past itself but are the products of a theoretical model. In short, it is often less accurate to say that the model is applied to the past than that the past is applied to the model. In the second place, the psychohistorical approach defines acceptable evidence more broadly than does traditional historical methodology. Traditionally, historians accept only evidence from the past, but psychohistorians, when they rely on theory, also accept evidence from the present to validate their interpretations. When they apply a psychoanalytic theory to a historical subject, they usually make no attempt to prove the theory's validity with evidence from the past. Instead, by citing psychoanalytic literature, they use contemporary evidence to prove the theory's validity. The assumption is made that all people are the same regardless of time and place and that, if a theory can be proved on the basis of contemporary evidence, it is considered proved for human beings of the past. Thus, psychological theory has become a general law, and contemporary data have become historical evidence.

Erik Erikson's creative and pioneering book *Young Man Luther* is still perhaps the most influential attempt to investigate the psychological dimension of history. It contains a particularly explicit and forthright statement of the psychohistorical method. Erikson intended to show how the resolution of Martin Luther's identity crisis took the form of his break with the Roman Catholic Church and his discovery of his own identity in a personal and a religious sense. Erikson's methodological approach is based on the assumption that—like the adolescents and young adults Erikson had observed in his clinical practice—Martin Luther experienced a crisis of identity "in that period of the life cycle when each youth must forge for himself some central perspective and direction, some working unity, out of the effective remnants of his childhood and the hopes of his anticipated adulthood."[6] Erikson made no systematic attempt to prove solely with evidence from the past that Luther experienced a crisis of identity or that such a phenomenon existed in Luther's time. Instead, he sought to pinpoint the moment in Luther's life when the crisis occurred and to work out its psychological, political, and religious implications. Erikson contended that evidence from the past is not by itself sufficient to enable the historian to come to a meaningful interpretation of Luther. "But a clinician's training permits, and in fact forces, him to recognize major trends even where the facts are not all available; at any point in the treatment he can and must be able to make meaningful predictions as to what will prove to have happened."[7] Even if the evidence from the past was questionable and came from unreliable sources, Erikson was prepared to accept it as long as it "does not contradict well-established

[6] Erikson, *Young Man Luther: A Study in Psychoanalysis and History* (New York, 1962), 14.
[7] *Ibid.*, 50.

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facts; persists in having a ring of truth; and yields a meaning consistent with psychological theory."[8] Hence, Erikson accepted Luther's "fit in the choir"—despite the uncertainty about whether it actually occurred—as pivotal in what he believed was Luther's crisis of identity. To be sure, not all of *Young Man Luther* is based on the psychohistorical method, and portions of the book represent a sensitive, and at times moving, attempt to understand the psychological dimension of Luther's theological reformation on its own terms. Nevertheless, by accepting the "fit in the choir" because it is consistent with psychological theory and by basing his historical understanding of Luther on the a priori assumption that Luther experienced an identity crisis, Erikson (like many psychohistorians who have followed him) used an approach that did not begin with his historical subject but with a contemporary theoretical model that provided historical understanding and explanation.

It is easy for historians to criticize both *Young Man Luther* and the method on which the book is based.[9] Yet Erikson's work should not be evaluated entirely as history. The book is a poetic and philosophical reflection on subjects ranging from tyranny and rebellion to creativity and human greatness. More important, *Young Man Luther* is a vehicle that Erikson used to popularize his views on identity and on the crisis of identity that often accompanies the transition from adolescence to adulthood. Even Erikson's psychohistorical approach cannot be dismissed as summarily as many critics have assumed.[10] It is undeniable that the universal plays a role in history. Despite all that divides peoples of different times and cultures, they are joined by some fundamental experiences, which are indispensable to the historian in his effort to understand the past. Erikson could have made a plausible case for the universality of the identity crisis, citing, for example, rites of passage ranging from Christian confirmation to the *chisunga* of the Bemba people in Zambia. Indeed, every historical interpretation, even the most traditional, presupposes the existence of certain human regularities and is based on contemporary assumptions and even contemporary evidence. These assumptions are so basic to us, so obviously true, so commonsensical, that we are largely unaware of them.

What distinguishes the psychohistorical from the traditional historical approach is ultimately a matter of degree. To be sure, no historian comes to the past without certain theoretical possibilities in mind. But there is a difference between the historian whose understanding, perhaps facilitated by theory, derives basically from history and the psychohistorian whose understanding is the product of a theoretical model that is imposed on the past. Of course, no historian approaches his subject unaffected by his contemporary situation. But there is a difference between the historian whose contemporary concerns influence his choice of subject and whose contemporary assumptions color and even facilitate his historical

[8] *Ibid.*, 37.

[9] For more on Erikson's psychohistorical method, see Erikson, "On the Nature of Psychohistorical Evidence: In Search of Gandhi," in Robert J. Lifton and Eric Olsen, eds., *Explorations in Psychohistory: The Wellfleet Papers* (New York, 1974), 42–77.

[10] See, for example, Roland H. Bainton, "Psychiatry: The Examination of Erikson's *Young Man Luther*," *Religion in Life*, 40 (1971): 450–78; and Lewis W. Spitz, "Psychohistory and History: The Case of *Young Man Luther*," *Soundings*, 56 (1973): 182–209.

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explanations and the psychohistorian who accepts uncritically the historical validity of contemporary evidence and uses it as a substitute for evidence from the past. Although the present may distort or inform the historian's understanding and explanations, he still attempts to transcend the limitations of his own time and to put himself as much as possible into the context of the past. By contrast, when employing the psychohistorical method, the psychohistorian applies the standards of the present to the past without attempting to take differing historical circumstances into account.[11]

There is also a difference, perhaps, between the application of theory to the past in the psychohistorical method and the application of theory in some of the newer social science approaches to history. This difference grows out of the nature of the dimension of the past that is being investigated. Consider, for example, the historian who applies a contemporary epidemiological model of the impact of smallpox on a previously unaffected population to account for the sudden drop in the population of the New World during the sixteenth century after the arrival of the first Europeans.[12] Clearly his understanding is the product of a medical theory that is imposed on the past, and the validity of his explanation is largely dependent on our acceptance of contemporary evidence. Perhaps we can assume that human physiology has not changed significantly in the last four hundred years and that human beings are likely to have the same response to the introduction of smallpox virus in the twentieth century as they had in the sixteenth. And yet even man's physiological response to disease is likely to be affected by cultural, social, and psychological factors. Customs, hygiene, nutrition, the state of medicine, the structure and nature of social intercourse, state of mind, and other factors susceptible to historical change can exercise a profound influence on the immune system and on recovery, death, and transmission rates of disease. Certainly in the case of human psychology, which is greatly influenced by changing familial and social environments, we cannot assume that people today and people of the past are in all respects the same psychologically. Even the basic psychological experiences that we share across time, given our common humanity, are certain to be expressed in a different psychological language and in response to different historical circumstances—differences that must make us regard the attempt to impose a psychological model on the past with considerable skepticism. In general, it seems evident that, although the use of contemporary theoretical models is probably justified when we are investigating those aspects of the past least susceptible to changing historical circumstances, employing contemporary theoretical models becomes more problematic when we are investigating those aspects most susceptible to change over time.

Obviously, then, there are acute problems of a methodological, even philosophical, character involved in the application of the psychohistorical method to history. And, as I suggested at the outset, many of the deficiencies of

[11] For further discussion of these distinctions, see pages 20 to 24 of this article and especially footnote 27.

[12] William McNeill, *Plagues and Peoples* (Garden City, N.Y., 1976), 1–14, 176–207.

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psychohistorical works can be traced to a reliance on that method. Some studies make no attempt at interpretation; "pathographies," descriptive psychiatric histories of the mental illnesses of great men of history, rely simply on diagnostic theory. These chronicles make little or no attempt to uncover the psychological dimension of a great man's historical significance, to account for his historical greatness. At worst, pathography is a form of character assassination, as a historical or contemporary public figure is discredited through the exposure of psychological weaknesses and symptomatology. At best, pathography is a form of pointless historical gossip as the intimate details of a public figure's personal life are exposed for their own sake. From the point of view of historical scholarship, information about the personal life of a historical figure should only be presented if that information either directly or indirectly has relevance for the understanding of his historical significance.

Even when diagnostic description has been transcended and historical interpretation attempted, the reliance on theory in the psychohistorical approach has often led to explanations that are brittle, superficial, and unconvincing. An extensive use of psychoanalytic jargon may mask an author's confusion, contradiction, and lack of psychological sophistication, although the reader may be impressed and intimidated. Finally, because the investigator who relies on the psychohistorical method usually organizes his understanding of a historical figure or event around a single narrow interpretative formulation based on a psychological theory, psychohistorical studies tend to be psychologically simplistic and historically reductionistic.[13]

THERE ARE TWO POSSIBLE EXPLANATIONS for the fact that some historians, in studying the psychological dimension of the past, have at times abandoned their traditional historical approach in favor of the psychohistorical method. Often encouraged, unfortunately, by psychoanalysts, historians have had the mistaken belief that to understand a historical figure psychologically on his own terms requires extensive information about his early life. When the information is not available, these investigators, like Erikson in writing *Young Man Luther*, turn to theory to provide from the present what is missing in the past. Actually the psychoanalyst is no more fortunate than the historian in possessing objective information about the early life of his subject. Contrary to common belief, the analyst learns nothing about the early lives of his patients. Instead, he learns a great deal about the memories—with all their distortion of the past and their reflection of the present—of his adult patients. The psychoanalyst does have one indispensable way of uncovering the psychological essence of his patients. By studying the transference as it is established during the course of an analysis, the psychoanalyst

[13] The reader is doubtless already familiar with examples of these psychohistorical "abuses." There is a significant difference, however, between the well-meaning and serious, if perhaps simplistic and reductionistic, attempt to understand the psychological in history and the psychohistorical exposé that can at times verge on historical pornography. For examples of the more frivolous and distasteful sort of psychohistory, see *The Journal of Psychohistory*. For more serious and scholarly attempts to understand the psychological dimension of the past, see *The Psychohistory Review*.

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is able to understand the thwarted childhood needs and frustrated childhood wishes as these are remobilized during the course of an analysis and "transferred" onto the figure of the analyst. The historian obviously must do without a direct transference from the person he is studying.

Nonetheless, it is erroneous to assume that the historian, having no transference to himself to study, must rely on theory and contemporary evidence for psychological understanding and explanation. In fact, the historical record does provide the researcher with transferences to study, those that the historical subject established to important persons in his life. Even more important, the course of an individual's life expresses the psychological essence of the person—his needs and wishes, his aims and ideals, his loves and hates, his conflicts, his basic transferences (in their original meaning of "transferences across the repression barrier"), the basic pattern of his self. To be sure, early life experiences play an important role in giving shape to the self of the individual, and a knowledge of these experiences can prove extremely helpful to the historian. But, even if the historian is forced to do without this knowledge, he can more than compensate for it by studying the entire life-curve of his historical subject, which expresses both the experiences of childhood and the subsequent experiences of adult life. The course of an individual's life, in other words, tells us more about the essence of his personality than do the facts of his childhood. Erikson would have written a more historically significant and psychologically revealing portrait of Martin Luther had he focused less on the few facts that are known or suspected about Luther's early life and more on Luther's adult theological writings, writings that express directly and tellingly Luther's religious and personal concerns, the theological and psychological essence of his personality.

A second explanation for the dependence of some psychohistorians on the psychohistorical method is that they may have a fundamental misconception about the nature of psychoanalysis as a clinical discipline. Their familiarity with analysis is usually based almost exclusively on reading psychoanalytic theory, indeed, on reading psychoanalytic metapsychology. They have little knowledge of analytic technique, clinical theory, and the nature of the psychoanalytic situation. They usually have no clinical experience or a sense of how a psychoanalysis actually operates, of how analysts think when treating their patients. Psychoanalysts have recommended analysis for historians interested in psychohistory so that they will avoid the problem of countertransference in their historical work, so that personal issues will not distort or inhibit their understanding of their historical subjects. But the principal advantage in having formal psychoanalytic training for historians interested in psychohistory is that they can learn, through their own analyses, through their exposure to the whole range of analytic literature, through having heard many cases presented and discussed in depth over a period of years, and through their own clinical experience with patients, how a psychoanalysis is actually conducted.

For, if the historian gains an understanding of clinical practice, he will discover that no good analyst, in treating his patients, does what psychohistorians routinely do when using the psychohistorical method. No good analyst reduces the problems

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of his patient—let alone his rich and complex personality—to a single interpretative formulation. No good analyst understands and explains his patient using the language or cognitive organization of an abstract theoretical construct, nor does he rely on evidence that he and other analysts have gathered in working with other patients either in formulating his understanding of his patient or in communicating that understanding to his patient. No good analyst invokes a general law in attempting to explain to his patient the reasons for his feelings, thoughts, and actions. A good psychoanalytic understanding must make sense to the analyst on the basis of what he has learned from the specific patient he is treating and must make sense to the patient in terms of the patient's experience of his own life.

The assertion that an understanding of clinical practice will lead historians to abandon what has been defined above as the psychohistorical approach may strike the reader as paradoxical, since practicing psychoanalysts are often the very ones who have written studies of history that rely so heavily on this method. Indeed, the studies on which psychohistorians have frequently modeled their work have often been written, like Erikson's *Young Man Luther*, by seasoned and gifted clinicians. How is one to account for this paradox? The explanation lies in the fact that analysts deal with history in a way that is fundamentally different from the way they treat their patients. As clinicians, analysts are concerned with their patients; as historians, they are not much concerned with history. Even when they avow that their aim is historical, psychoanalysts approach the past for their own psychoanalytic purposes. History is not of interest for its own sake; rather, it is used to popularize or confirm current psychoanalytic theories.

Therefore, the psychoanalyst in his clinical practice has little in common with either the psychoanalyst or the psychohistorian using the psychohistorical method. The methods used by clinicians are for the most part the same as those traditionally used by historians. Just as the historian appreciates the complexity of the past, so the psychoanalyst recognizes the complexity of his patient, the many factors that influence him, the many different motives he may have. Just as the historian understands an event of the past as being the product of a multitude of causal factors, historically comprehensible only from many different perspectives, so the psychoanalyst understands his patient in many different ways, from many different points of view, offering literally hundreds of different interpretations during the course of an analysis. And, just as the historian focuses on a particular past event and seeks to understand it in depth and on its own terms, so the psychoanalyst focuses on the experience of his particular patient and seeks to understand him in depth and in terms of his own unique language and experience.

The works of colleagues, past and present, help the historian investigate the particular aspect of the past that interests him. The contributions of others to his own field of investigation assist the historian in defining his subject, in raising relevant and historically significant questions, in suggesting a variety of possible historical answers to those questions. The contributions of colleagues investigating related fields, including those of other disciplines, may similarly stimulate and assist the historian in his area of inquiry, as he compares their conclusions with his own tentative hypotheses. Theory, in the clinical practice of psychoanalysis, serves

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a comparable function for the analyst. Although those analysts who regard psychoanalytic theory less as a clinical tool and more as the basis of a general human psychology will perhaps disagree, theory in psychoanalysis represents not so much a compendium of general laws as a systematic attempt to organize and communicate the concrete clinical experience of many different analysts.[14]

Theory is necessary in psychoanalysis because each analyst can have direct clinical experience of only a relatively small number of patients. Theory exposes the analyst both to a large number of patients and to the knowledge and experience of colleagues, past and present, who have worked with them, thereby increasing what he can understand about his own patients. But theory in analysis should never be imposed from without on the patient or intrude on the analyst's free-floating attention. Should theory intrude on the analyst's thinking, it becomes a countertransference, an interference with the analyst's ability to understand his patient. Therefore, just as the work of other scholars aids the historian in coming to his own understanding of his particular historical subject, so psychoanalytic theory (psychological generalization) aids the analyst in coming to his own understanding of each patient (the psychological particular). But the work of other scholars can never provide the historian with his historical understanding, nor can psychoanalytic theory ever provide the analyst with his psychological understanding. Ultimately, in both history and psychoanalysis, the particular is the focus of attention, and generalization in both disciplines is only a tool to help the investigator comprehend the particular on its own terms.

The comparable relationship between general and particular in history and in the clinical practice of psychoanalysis reflects the fact that both disciplines, to the extent that they seek to understand human feelings, thoughts, and actions, share what the philosopher Wilhelm Dilthey defined as the methodology of the human sciences—that is, knowing by empathic understanding. The essential element in the psychoanalytic and historical understanding of human beings is, in Dilthey's well-known phrase, "the rediscovery of the I in the Thou." In both disciplines the investigator feels or, perhaps better, thinks his way inside the experience of the other when he understands him. Unfortunately, "empathy" has come to be confused in popular usage with such words as "sympathy" and "compassion." In fact, empathy is our ability to transcend subjective responses and to experience, although to a necessarily attenuated degree, the experience of another. In both disciplines understanding is achieved when the investigator puts himself in the place of the other and imagines why—given the time, place, circumstance, and experience of the other—it makes sense that he felt, thought, and acted as he did.

Explanation in both disciplines is offered in the form of interpretation based on available evidence, although there are differences in how psychoanalysts and

[14] For the view of psychoanalytic theory as a compendium of general laws, see, among others, the work of Charles Brenner, Heinz Hartmann, and David Rapaport. For a view similar to mine of the role of psychoanalytic theory, see Merton Gill, "Metapsychology is not Psychology," in *Psychological Issues, Monograph 36* (New York, 1976), 71–105; and George M. Klein, "Freud's Two Theories of Sexuality," in *ibid.*, 14–70, and "Two Theories or One?" *Bulletin of the Menninger Clinic*, 37 (1973): 102–32.

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historians assess the validity of an interpretation. In history, the reader's acceptance of an explanation is determined by the inherent logic of it and by the relationship between the explanation and the evidence, that is, whether the evidence adduced supports the explanation or, better, whether the explanation accounts for the evidence. In other words, does the interpretation have explanatory power? Although these factors play a role in psychoanalysis, the patient's acceptance of an explanation is determined primarily by whether he feels understood by it, whether it comprehends his experience of himself.

Despite these differences, explanation in both psychoanalysis and history seeks to make the subject empathically understandable. In psychoanalysis, the analyst seeks through his interpretations to help the patient achieve empathy with himself, to give him an understanding of why—given his past and present, given his experiences and environment—it makes sense that he feels, thinks, and acts as he does. In history, the historian seeks through his interpretations to make the reader empathize with the historical subject, to show the reader why—given the historical subject's experiences and circumstances—it makes sense that what happened did happen. What so often passes as "commonsense explanation" in history is thus explanation that is able to engender empathic understanding on the part of the reader.[15] In other words, the power and, indeed, the validity of explanation in psychoanalysis and history depend, to some degree, on the investigator's ability to communicate empathic understanding in such a way that the person being addressed—be he patient or reader—can also empathically understand the subject.

Despite its central importance in psychoanalysis and history, the position of the empathic observer does not, of course, define the methodology of either discipline completely, and there are times when analysts and historians step out of the empathic position. In making a diagnosis, for example, the psychoanalyst does not empathize with his patient. Instead, he functions as an external observer able tentatively to assess the nature of the patient's psychopathology, his psychological strengths and weaknesses, and his suitability for psychoanalysis or some other form of therapy. Similarly, in assessing the historical significance of his subject, the historian steps outside of his subject's experience in order to evaluate from an external vantage point the historical impact of his subject's actions. Moreover, because the empathic approach comprehends only human experience, the role of empathy is further reduced in history, which studies not only people but also their creations (their institutions and economic systems), aspects of the past probably not accessible by empathy.

Analysts who regard passing judgment on their patients as a significant part of the psychoanalytic process restrict the role of empathy even further. Specifically, the analyst concerned with the capacity of his patient "to test reality" takes himself

[15] See, for example, Barzun, *Clio and the Doctors*. Although much of Barzun's critique of the psychohistorical method is justified, his rejection of all psychohistory is based on a misunderstanding of the nature of psychoanalytic methodology. In fact, Barzun's definition of the historical approach is similar to that presented in this article. History so defined is fundamentally compatible with the clinical practice of psychoanalysis.

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out of the position of the empathic observer and sets himself up as an external observer able to judge which of his patient's feelings, thoughts, and actions are appropriate and realistic and which are not.[16] But, for the most part, it is not the psychoanalyst's task to function as a judge, and the psychoanalytic method rarely requires the analyst to adopt the stance of an external observer. From the point of view of the patient's experience—that is, from the empathic point of view—the patient's feelings, thoughts, and actions are always appropriate and in accord with reality, namely, the reality of the patient's psychic life.[17]

Similarly, historians who regard passing judgment on the past as a significant part of the historical enterprise and historians who seek to give scientific respectability to history minimize the methodological importance of empathy. The former, instead of thinking themselves inside the experience of their historical subject, set themselves up as external observers able to determine how their subject should have behaved. The latter, essentially the logical positivists and members of the analytical school, reject the empathic approach as unscientific and insist that the investigator of the past adopt the stance of an external observer who restricts historical knowledge to positive causal explanation. From his external perspective, the historian's task then becomes the formulation of covering laws or, if these are assumed to be implicit in historical explanation, simply the description of causal sequences.[18] But, as is the case with both the analyst and his method in psychoanalysis, the historian's task is generally not to function as judge or scientist, and the historical method rarely requires the historian to adopt the stance of an external observer. Moral judgment and the formulation of covering laws are difficult to reconcile with historical change; the questions "how ought" and "how does" are unhistorical. And the historian should not abandon the search for historical meaning in favor of positive historical truth by restricting himself only to the question "how did." The most significant question the historian can answer

[16] See, for example, N. Treurniet, "Discussion of Paper by Dr. Lichtenberg," in Joseph D. Lichtenberg and Samuel Kaplan, eds., *Reflections on Self Psychology* (Hillsdale, N.J., 1983), 187–202.

[17] Of course, a patient can experience his own feelings, thoughts, and actions as inappropriate. Under these circumstances, the analyst's task is to empathize with the patient's experience of inappropriateness as well as with those feelings, thoughts, and actions that have given rise to it. For more on the empathic basis of the psychoanalytic methodology, see Heinz Kohut, *The Analysis of the Self: A Systematic Approach to the Psychoanalytic Treatment of Narcissistic Personality Disorders* (London, 1971), 300–07, *The Restoration of the Self* (New York, 1977), 298–312, "Introspection, Empathy, and Psychoanalysis: An Examination of the Relationship between Mode of Observation and Theory," "Forms and Transformations of Narcissism," "Psychoanalysis in a Troubled World," "The Psychoanalyst in the Community of Scholars," in Paul H. Ornstein, ed., *The Search for the Self: Selected Writings of Heinz Kohut, 1950–1978*, 2 vols. (New York, 1978), 1: 205–32, 427–60, and 2: 511–46, 685–724, "Letter to a Colleague, September 1978," "Reflections on Advances in Self Psychology," in Arnold Goldberg, ed., *Advances in Self Psychology* (New York, 1980), 456–69, 473–554, and "Introspection, Empathy, and the Semi-Circle of Mental Health," *International Journal of Psycho-Analysis*, 63 (1982): 395–407; and Paul H. Ornstein, "Remarks on the Central Position of Empathy in Psychoanalysis," *Bulletin of the Association for Psycho-Analytic Medicine*, 18 (1979): 95–108.

[18] See, for example, Carl G. Hempel, "The Function of General Laws in History," in Herbert Feigl and Wilfrid Sellars, eds., *Readings in Philosophical Analysis* (New York, 1949): 459–71; and Patrick L. Gardiner, *The Nature of Historical Explanation* (Oxford, 1952).

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is "why." To answer that question, empathic understanding remains the only viable approach, despite the passage of one hundred years since Dilthey first described it.[19]

IN CONTRAST, THEN, TO THE PSYCHOHISTORICAL METHOD, which relies on theory to provide its understanding and explanation, both the historical and the clinical psychoanalytic approach rely on empathy to understand and explain the particular meaning of human subjects.[20] This fundamental methodological unity makes clear that, in studying the past's psychological dimension, the historian can remain a historian; he can continue to use traditional historical methods and, in doing so, function in a way that is fundamentally compatible with the way in which the psychoanalyst functions as a clinician.[21]

[19] The philosophical debate over the role of empathy in historical epistemology transcends the limits of this article and has only been touched on briefly. This cursory treatment of such an extraordinarily contentious and important problem is justified, however, because this article seeks only to demonstrate that history and psychoanalysis attempt to understand and explain people in essentially the same way. Whether this method is fundamentally flawed or not is irrelevant in the present context. This article does not address the question of whether psychohistory properly practiced (as the historical investigation of the psychological dimension of the past), psychoanalysis, and history succeed or fail epistemologically. It seeks only to demonstrate that they succeed or fail together. For a nonpositivist critique of empathy, and especially a critique of Dilthey, see Hans-Georg Gadamer, *Wahrheit und Methode* (Tübingen, 1965), 205–28. Also see Jürgen Habermas, "Review of Gadamer's Truth and Method," in Fred R. Dallmayr and Thomas A. McCarthy, eds., *Understanding and Social Inquiry* (Notre Dame, Ind., 1977), 335–63. A critique of the empathic approach is also implicit in the writings of Jacques Derrida and Michel Foucault. For an attempt to place empathic understanding within an objective scientific framework, see the work of Max Weber. In particular, see Weber, "Roscher und Knies und die logischen Probleme der historischen Nationalökonomie," in Weber, *Gesammelte Aufsätze zur Wissenschaftslehre* (Tübingen, 1951), 67–126, " 'Objectivity' in Social Science and Social Policy," in Edward A. Shils and Henry A. Finch, eds., *Max Weber on the Methodology of the Social Sciences* (Glencoe, Ill., 1949), 50–112, and "Critical Studies in the Logic of the Cultural Sciences," in Shils and Finch, *Max Weber on the Methodology of the Social Sciences*, 113–88, esp. 176–77. In addition to Dilthey, Georg Simmel, a philosopher of essentially the same era, assigned a significant role to empathy in historical understanding. See his *Die Probleme der Geschichtsphilosophie* (Leipzig, 1892) and *Vom Wesen des historischen Verstehens* (Berlin, 1918). For an Anglo-Saxon version of empathic understanding, see R. G. Collingwood's explanation of historical knowledge as the re-enactment of past experience in the mind of the historian; Collingwood, *The Idea of History* (Oxford, 1972), esp. 282–302. Finally, for a more contemporary analysis of historical knowledge in this tradition, see William Dray, *Laws and Explanation in History* (Oxford, 1970).

[20] Among the significant differences between history and psychoanalysis, and not the least of them, to paraphrase Peter Loewenberg, is that the historian seeks merely to understand the story, whereas the psychoanalyst seeks also to help his patient change the story's ending. Furthermore, whereas the historian studies human beings in the past, the psychoanalyst studies human beings in the present. In contrast to the historian and his subject, the analyst has the opportunity to observe both the patient's transferences to him and the impact of his interpretations on the patient. But the ability to observe the patient's transferences and reactions, although an essential part of the therapeutic process, does not serve to distinguish psychoanalysis and history methodologically. Neither the patient's transferences nor his reactions affect how the analyst knows; they only affect what he knows. The primary difference between psychoanalysis and history is thus one of subject, not method. The psychoanalyst understands the patient's relationship to him in the same way he understands every other experience of the patient in the past and present—in the same way the historian understands his historical subject—that is, empathically.

[21] Perhaps not by accident, Peter Loewenberg, another historian with clinical experience, has also stressed the methodological similarity of history and psychoanalysis, particularly their common subjectivity. Indeed, Loewenberg argued that historians can learn from psychoanalysts how to use the discovery of transference and countertransference to enrich historical understanding. See, particularly, Loewenberg, "On Psychohistory: A Statement on Method," in Peter Loewenberg, *Decoding the Past: The Psychohistorical Approach* (New York, 1983), 3–8, and "Psychoanalysis and History: The Scope

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Although this methodological compatibility makes it unnecessary for the historian to learn from the psychoanalyst a new approach to the psychological dimension of the past, this compatibility does not mean that psychoanalysis has nothing to offer historians. To the contrary, the depth of knowledge about human psychology that psychoanalysis provides can prove valuable to the historian precisely because of this methodological compatibility.[22] Psychoanalytic theory, if used by the historian in the way the analyst uses theory in the clinical situation, can enrich the historian's empathic understanding of the past and lead to work that is both psychologically sophisticated and historically significant.

The question whether historians interested in psychohistory need formal psychoanalytic training must remain open for some time to come, given that historians have only recently attempted to investigate systematically the past's psychological dimension. Clearly, practical considerations are involved; very few historians have the financial resources, the time, or the opportunity to become full-time candidates at a psychoanalytic institute. Furthermore, some historians possess natural empathy and an intuitive grasp of human psychology and can write psychologically sophisticated historical studies without the benefit of formal psychoanalytic education. It may also be possible for some historians to investigate the psychological dimension of the past based simply on their reading of psychoanalytic literature. Perhaps a better course for the historian interested in psychohistory is to undertake an intensive collaboration with an experienced psychoanalytic clinician. Some of the better studies of psychohistory have been based on just such collaborative efforts, and a literature on this particular approach is developing.[23]

of the Problem," in *ibid.*, 9–41. For a critique of the usefulness of the transference model to psychohistory, see Fred Weinstein, "The Transference Model in Psychohistory: A Critique," *Psychohistory Review*, 5 (1977): 12–17. For Weinstein and Gerald W. Platt's thoughtful attempts to emphasize the dissimilarity of history and psychoanalysis (both are sympathetic to psychohistory), see Weinstein and Platt, "The Coming Crisis in Psychohistory," *Journal of Modern History*, 47 (1975): 202–28, and *Psychoanalytic Sociology: An Essay on the Interpretation of Historical Data and the Phenomena of Collective Behavior* (Baltimore, Md., 1973). In addition to those already cited, a number of works have fruitfully addressed, directly or indirectly, some of the issues raised in this article. In particular, see Peter Gay, *Freud for Historians* (New York, 1985); William McKinley Runyan, *Life Histories and Psychobiography: Explorations in Theory and Method* (New York, 1982); Donald P. Spence, *Narrative Truth and Historical Truth: Meaning and Interpretation in Psychoanalysis* (New York, 1982); and Saul Friedländer, *History and Psychoanalysis* (New York, 1978). Among the many valuable articles on the methodology of studying psychohistory, see James W. Anderson, "The Methodology of Psychological Biography," *Journal of Interdisciplinary History*, 11 (1981): 455–75, and "Psychobiographical Methodology: The Case of William James," in Ladd Wheeler, ed., *Review of Personality and Social Psychology*, 2 vols. (Beverly Hills, Calif., 1981), 2: 245–72; Faye Crosby, "Evaluating Psychohistorical Explanations," *Psychohistory Review*, 7 (1979): 6–16; William J. Gilmore, "The Methodology of Psychohistory: An Annotated Bibliography," *Psychohistory Review*, 5 (1976): 4–33; and R. L. Schoenwald, "Using Psychology in History: A Review Essay," *Historical Methods Newsletter*, 7 (1973): 9–24.

[22] Those historians who are more positivistically inclined doubtless find behaviorist psychology most congenial. See, for example, Stannard, *Shrinking History*.

[23] See, for example, George Moraitis, "A Psychoanalyst's Journey into a Historian's World: An Experiment in Collaboration," *Annual of Psychoanalysis*, 7 (1979): 287–320; George Moraitis and Carl Pletsch, "Psychoanalytic Contribution to Method in Biography," *Psychohistory Review*, 7 (1979): 46–50; and Samuel H. Baron and Carl Pletsch, eds., *Introspection in Biography: The Biographer's Quest for Self-Awareness* (Hillsdale, N.J., 1985).

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There can be no question, however, that formal psychoanalytic training greatly increases the historian's sophistication. By studying psychoanalysis, the historian can increase his sensitivity to human needs and wishes. He can learn how people feel what they feel, understand some of the reasons why they may feel as they do, and learn to appreciate the power of emotion and what the consequences of emotion may be. Psychoanalytic training enhances the historian's capacity to understand empathically the people he studies and enhances his capacity to explain those people in such a way that his readers can empathize with them also. Indeed, even if he must remain a historian when he uses psychoanalytic theory, theory is a valuable tool that promotes the understanding and explanation of the past's psychological dimension.

How historians choose to present psychological theory in their work is perhaps ultimately a matter of personal taste. Some historians may be inclined to omit any mention of theory, even if it proved useful to them, although in the interest of intellectual honesty the historian is obliged to give credit to a psychological model if it helped him understand and explain his subject. Some historians may simply choose to relegate theory to the footnotes, thereby giving credit where credit is due but leaving the text to stand or fall on its own merits like other historical works. Nevertheless, a theoretical model can prove as helpful to the reader as to the investigator, and the historian, by presenting the theoretical models he used at the outset of his book or article, can enable the reader to keep the same psychological generalizations in the back of his mind that assisted the author. Finally, the historian may even choose to use theory to structure his account, to organize his thinking and presentation. No matter how theory is presented, however, the past must stand on its own, must be comprehended and made comprehensible on its own terms. Contemporary evidence cannot substitute for historical evidence. Understanding and explanation must remain thoroughly historical, that is, they must derive from the past itself.

Although common in psychoanalytic publications, personal experiences are rarely, if ever, related in historical journals. With the reader's indulgence, I will illustrate the role that psychological theory can play in facilitating empathic understanding in both psychoanalysis and history by citing two examples from my experience as a clinician and a historian. While in training as a research candidate at the Cincinnati Psychoanalytic Institute, I saw a middle-aged businessman twice a week in psychotherapy. He quickly became attached to me and, since he had often felt abandoned by his parents as a child, was fearful that I, too, might desert him. My vacations were to him a source of particular anxiety, which gradually diminished as he grew more secure in the knowledge that I would return from them. After about a year and a half of treatment, I told him that I would be out of town for approximately two weeks. Initially, the patient accepted this news with apparent equanimity, but within a short time he developed a cough so severe that he was forced to leave the session—something that had never happened before—to get a drink of water. He returned to my office after an absence of a few minutes. I recall feeling somewhat uncomfortable sitting alone in my office waiting with the door open for my patient to return. My discomfort disappeared, however, when

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I understood the meaning of my patient's unexpected behavior. My understanding of my patient was facilitated in this instance by what I had learned in my classes, was facilitated, in other words, by my knowledge of psychoanalytic theory. I was aware that a patient often reacts strongly to the announcement of his therapist's vacations and that in the case of a patient who had felt abandoned in early life this reaction was likely to be particularly strong. I was also aware that when people feel vulnerable, frightened, and abandoned, they tend to respond in kind, to do to others what they feel has been done to them.

I was not fully conscious of these theories as I sat alone in my office waiting for my patient's return, but my sensitivity to these issues clearly played a role in helping me arrive at my understanding of him. In this instance, psychoanalytic theory helped make understanding possible. Nevertheless, my understanding still had to remain empathic, still had to stand on its own, to make sense in terms of what I had learned about my particular patient and his background during the course of our work together, and still had to offer a sensible explanation for what had just transpired. I suggested to the patient that by leaving the office he was attempting to communicate to me how he would feel when I left on my vacation. He would be left waiting for me to return just as I had been left waiting for him to return from the drinking fountain. The fact that the patient felt understood by my interpretation, that he was able to express his feelings about my coming vacation and about similar feelings of abandonment in the past, is not directly relevant to the present discussion of methodology. What is important is how I arrived at my understanding, how, with the aid of psychological theory, I was able to put myself (or, perhaps I should say, allow myself to be put) in the patient's place, and how I attempted to understand and explain his actions empathically, that is to say, from the vantage point of his experience.

More than a year after this episode, while writing a study of Kaiser Wilhelm II that focused on the psychological dimension of his leadership, I came across an event in the summer of 1903 during one of his North Sea cruises that typified many of Wilhelm's reactions as Kaiser. The news arrived on board the royal yacht that the German South Pole expedition of that year, an expedition that had aroused the Kaiser's liveliest interest, had ended in failure. Wilhelm II reacted with disappointment and anger. He felt, according to his friend Philipp Eulenburg, who was with him at the time, "insulted" by the expedition's failure, and he ordered that material be collected in order "to crush" the expedition's unfortunate leader, Erich von Drygalski.[24] It seems clear that Eulenburg understood both the intensity and much of the meaning of Wilhelm II's outburst. Indeed, Eulenburg offered the interpretation that the Kaiser experienced the expedition's failure as something deeply humiliating. On the basis of my years of study of Wilhelm II's personality and behavior, I had no doubt that Eulenburg's interpretation was essentially correct. Nonetheless, I wondered whether a deeper understanding of Wilhelm's reaction was not possible. Based on my knowledge of the Kaiser and how

[24] Eulenburg to Chancellor Bernhard von Bülow, July 26, 1903, Federal Republic of Germany, Bundesarchiv Koblenz, Eulenburg Papers, vol. 74, "Nordlandreise II: Psyche."

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he had reacted in other similar circumstances and recalling, perhaps, the reaction of my patient to the news of my coming vacation, it occurred to me that a mechanism may have been operating with the Kaiser similar to the mechanism that had operated with my patient. I wondered whether Wilhelm II's wish "to crush" (*zerschmettern*) Drygalski might not reflect how he himself experienced the failure of the expedition, that is to say, he was "crushed" by it. Identifying himself intensely with the fortunes of the nation he led, the Kaiser was personally devastated by the failure of an expedition that he had anticipated would enhance German national pride and, by extension, his own self-esteem. This blurring in the mind of the Kaiser of the psychological boundary between himself and his country—as illustrated by this episode—would have fateful historical consequences both for Wilhelm II and for Germany.[25]

In the actions of my Cincinnati patient and of the last German Kaiser—two individuals who could hardly differ more in terms of time, place, and circumstance—a human reaction is manifested that is not infrequently encountered, namely, a wish to do to others what has been experienced as having been done to the self. This reaction is not a universal law; it is not true for all people at all times. But it does represent a shared human response, a psychological generality, that can be expressed in a number of theories, including "identification with the aggressor," "turning passive into active," and "the attempt to communicate how one has felt treated in the past and in the present." This last formulation is closest to my understanding of both my patient and Wilhelm II. My patient sought to communicate to me how he would feel during my absence. The Kaiser sought, perhaps, to communicate to Eulenburg and the others aboard the royal yacht how devastated he felt at the news of the failure of the South Pole expedition. The theory that people often respond in this way did not provide me with my understanding of the businessman or of the Kaiser. It did not prove that my understanding of either was correct. Instead, the theory (what I had learned about people in general) helped me arrive at an understanding and an explanation that made sense in terms of what I knew about these two people in particular. Acceptance of my interpretations, in turn, was not determined by theoretical considerations but by, first, whether my interpretation made sense to my patient as he observed himself in treatment, whether it fit with his own experience of himself, and, second, whether my explanation made sense to the reader of my study of Wilhelm II in terms of the experience and knowledge of the Kaiser that he had gained from me and from other historians. In both the clinical and the historical realm, then, theory remains only a tool—albeit a most useful one. The house must still be constructed out of the materials at hand.[26]

[25] For a detailed consideration of these issues, see Thomas A. Kohut, "Mirror Image of the Nation: An Investigation of Kaiser Wilhelm II's Leadership of the Germans," in Charles B. Strozier and Daniel Offer, eds., *The Leader: Psychohistorical Essays* (New York, 1985), 179–229.

[26] At the risk of repetition, let me spell out the differences between my use of theory in studying Kaiser Wilhelm II and the use of theory in the psychohistorical method as I have defined it. In contrast to the psychohistorian's theoretical model, which is located at the center of his consciousness, my theoretical knowledge was located in the back of my mind, at the edge of my awareness, and took the form less of a consciousness of theoretical models than of a sensitivity to certain psychological issues.

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IN CONCLUSION, HISTORIANS neither need to nor should depend on the psychohistorical method in studying the past's psychological dimension. That dimension, however, remains a historical subject of decisive importance. It is axiomatic that feelings play a major role in motivating people today and in the past. Events like the Reformation, the persecution of witches in seventeenth-century Salem, the building of the German high seas fleet before the First World War, the Holocaust, the Iranian hostage crisis, and the war over the Falkland/Malvinas Islands are simply incomprehensible without reference to their psychological dimensions.[27] The decisive importance of emotion in human life and in human history is unquestionable. There is a psychological dimension to every historical event, to every historical figure. There are no "irrational" psychohistorical subjects and no "rational" historical subjects without a psychological dimension.

Because it is not possible to comprehend people without dealing with the psychological, historians, including those critical of psychohistory, have always written about it, even if they have rarely acknowledged the fact. Hajo Holborn, for example, commented in *A History of Modern Germany* on Wilhelm II's "love-hatred" for England.[28] This simple characterization of what was an extraordinarily subtle and complex attitude on Wilhelm's part is not spelled out or supported with evidence in Holborn's book.[29] Nor is Holborn's characterization based on a systematic and rigorous monographic treatment of the psychology of the Kaiser's relationship toward England written by him or by any other serious scholar. Holborn's comment about the Kaiser's ambivalence is an easy and superficial psychological generalization, written by a careful, thoughtful, and experienced

In contrast to the psychohistorian who is able to have access to only one or two theoretical models at any given moment (because of their location at the center of his consciousness), I was able to have access to many different theoretical models that (because of their location in the back of my mind) could be called forth at any given moment in response to the historical material I was investigating. In contrast to the psychohistorian who may use theory in the service of what Erikson called "retroactive reconstruction" (see, for example, his reconstruction of Luther's "fit in the choir"), I used theory to facilitate my understanding of a well-documented historical event. In contrast to the psychohistorian whose interpretation is validated when based on and consistent with psychological theory, my interpretation, although facilitated by psychological theory, was validated when based on and consistent with the evidence from the past. My understanding of the Kaiser was expressed in everyday language, and the theory that facilitated my understanding was mentioned only as an afterthought, not to prove that my interpretation was correct but to give credit where credit was due. In summary, whereas the psychohistorian imposes theory on the past and relies on contemporary evidence to validate his interpretation, I used theory only to facilitate my empathic understanding of the past and relied only on historical evidence to validate that understanding.

[27] In this article, I have focused primarily on the attempt to understand historical individuals rather than groups. This emphasis reflects the fact that most work on psychohistory has focused on individuals and that perhaps the most promising way to begin to understand the psychology of a group in history is to explore the psychology of its individual members. For a pioneering attempt to understand the psychological dimension of a historical group, see Peter Gay's *The Bourgeois Experience: Victoria to Freud*, a study of the bourgeoisie in Western Europe and America from the 1820s to 1914. Two volumes of what is projected to be a multivolume work have already appeared. See Gay, *Education of the Senses* (New York, 1984), and *The Tender Passion* (New York, 1986).

[28] Holborn, *A History of Modern Germany: 1840–1945* (New York, 1969), 305.

[29] For a systematic attempt to understand and explain the Kaiser's confusing attitude toward England, see Thomas A. Kohut, "Kaiser Wilhelm II and his parents: an inquiry into the psychological roots of German policy towards England before the First World War," in John C. G. Röhl and Nicolaus Sombart, eds., *Kaiser Wilhelm II: New Interpretations* (Cambridge, 1982), 63–89.

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scholar, apparently without any sense that the observation begged for elucidation and evidence.

In writing about the psychological in history, the historian should rely neither on the psychohistorical method nor on unsubstantiated psychological speculation. Historians need to exercise the same rigorous scholarship in writing about history's psychological dimension that they exercise in writing about other aspects of the past. Furthermore, just as economic history is enriched by a knowledge of economics, legal history by a knowledge of the law, and political history by a knowledge of politics, so psychohistory is enriched by a knowledge of human psychology. In other words, the historian should write what Peter Gay has called "history informed by psychoanalysis"—history that is knowledgeable about people, that is psychologically sophisticated, but history that is history through and through.

Even if historians approach the psychological dimension of the past using traditional historical methods, psychohistory will remain controversial. But the controversies will simply be part of the wider debates over subject and method that divide and enliven the historical profession. Psychobiographical studies will be included in the debate over the role of the individual in history. But it is no more and no less difficult to understand the psychological concerns of an individual and to define his psychological impact on the course of history than it is to understand that individual's political and intellectual concerns and to define his political and intellectual influence. Psychosocial studies will be included in the debate over whether and how the historian can generalize about the thoughts, feelings, and actions of groups in history. But it is no more and no less difficult to identify and understand popular feelings than it is to identify and understand popular political opinions, economic motives, and social attitudes. And psychohistory will be included in the debate over historical epistemology. But psychohistory will be no less positive or empirical, no less relative, and no more clearly defined or logical than any other sort of history. Psychohistory will be part of what historians, sensing the ill-defined nature of their method, are wont to call the historian's craft. In short, psychohistory will be history, and psychohistorians will be historians once again.

In the 1830s, Leopold von Ranke wrote that it was not necessary or desirable "to explain the observable [historical] actions of individuals as the result of the passions which we derive deductively from our concept of man. The resulting point of view is tinged with an aridity, irreligiosity, and lack of sensitivity which drive us to despair." Rather than impose a contemporary conception of man on the past, Ranke argued that "we have to investigate the genuine information as precisely as possible to determine whether we can discover the real motives. Doing so will be possible more frequently than one might think." Ranke concluded that we lose nothing by abandoning the attempt to impose a theoretical conception of man onto history; "no, the more documentary, the more exact, and the more fruitful the research is, the more freely can our art unfold, which flourishes in the element of immediate, undeniable truth. Only invented motives are dry. The true

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ones, derived from fresh observation are diverse and profound."[30] I have sought to make essentially the same point. To be sure, Ranke was unaware of historical relativism and overconfident about our ability to discover historical truth. Nonetheless, although we can never achieve our goal, the effort to understand and explain the psychological dimension of the past historically—that is, as much as possible on its own terms—can lead us closer to "real motives," motives that are "diverse and profound," rather "more frequently than one might think."

[30] Leopold von Ranke, "On the Character of Historical Science," in Leopold von Ranke, *The Theory and Practice of History*, ed. Georg G. Iggers and Konrad von Moltke (Indianapolis, Ind., 1973), 41.

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Reviews of Books

GENERAL

CHARLES B. STROZIER and DANIEL OFFER, editors. *The Leader: Psychohistorical Essays*. Foreword by PETER GAY. New York: Plenum. 1985. Pp. xvi, 324. $39.50.

This anthology is reminiscent of the little girl with the curl. When it is good, it is very good indeed; when it is bad it is appalling. The tedious introductory chapters by Charles B. Strozier and Daniel Offer add little to our understanding of the problems of applying psychology to historical analysis. The editors resent the criticisms leveled at "psychohistorians" by their more conventional colleagues, but their own ventures into history demonstrate precisely why the genre sometimes lies open to attack: their essays abound in reductionism, irresponsible overstatement, and historical misunderstanding.

In "Lincoln and the Crisis of the 1850s" Strozier asserts that Lincoln's personal paranoid tendencies responded to the "rampant paranoia" sweeping America: "Something new appeared on the scene in the 1850s. The dark shadow of paranoia . . . came to blot out the sun entirely . . . Lincoln shuddered in terror at an imagined conspiracy" (pp. 92, 93). The language is extravagant, and the evidence is insufficient to persuade us that the clinical diagnosis of paranoia is appropriate. One can certainly find evidence of suspicion, violence, and conspiracy in the 1850s, but they scarcely constitute "rampant paranoia" and were not "something new" in American life. As Leonard Richards has shown, hysteria, suspicion, and mob violence against abolitionists reached their apogee in the 1830s, and the irrational nativist, anti-Negro hysteria burst into violent riots in Philadelphia in the 1840s. Moreover, Lincoln did not reenter politics in 1854 because he had suddenly "become a different person" (p. 102); such a statement is psychologically naive and historically ill informed. Lincoln reentered politics for solid political reasons and not because of any undisclosed psychic metamorphosis. By insisting on psychological explanations without sufficient documentation, Strozier trivializes political, social, and economic issues of transcending importance.

"Leaders and the Arab-Israeli Conflict: A Psychoanalytic Interpretation" by Offer and Marvin Zonis offers no meaningful psychological or historical interpretation. Instead of presenting a systematic study of leaders in the Near East, the authors flit about among a dozen topics and offer such startling inanities as "We must listen to what people say to understand what they are saying" (p. 293) and "One cannot get a meaningful psychological understanding of the Middle East by studying it at a distance, . . . or even in the region" (p. 294).

In their concluding chapter, "Reflections on Leadership," Strozier and Offer include the following: "The ability to get along with others is important for most persons in office" (p. 308), and twentieth-century political leaders are "somewhat more stable emotionally and less impulsive than leaders of the past millennium" (p. 309). (They do not use Adolf Hitler, Idi Amin, Jean Bokassa, and Muamar el-Quaddhafi to support their thesis.)

It is regrettable that the seminal ideas of Heinz Kohut, which can prove valuable to historians, have been inappropriately applied in such superficial and ill-considered articles. We can be grateful, however, that three essays are strong enough to rescue this volume from disaster: Human Muslin and Prakash Desai's "The Transformation of the Self of Mahatma Gandhi," Joseph Bongiorno's "Woodrow Wilson Revisited: The Pre-Political Years," and Thomas Kohut's "Mirror Image of a Nation: An Investigation of Kaiser Wilhelm II's Leadership of the Germans."

In their penetrating and persuasive article Muslin and Desai fulfill their purpose of providing "not a solution to the mystery of Gandhi, but . . . an enhanced appreciation of the complexities of the man" (p. 111). They employ Kohutian analysis to reach a deeper insight than Erikson was able to achieve. The young Gandhi, a distraught and fragmented youth, engaged in an anxious, lifelong search for "the perfect Guru"—which is to say, for an idealized parent or for some purpose that would give mean-

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ing and cohesion to his disjointed self. The authors believe that a humiliating and traumatic incident on a train in South Africa produced "a dramatic and lasting self-transformation" (p. 129), enabling Gandhi to channel his "narcissistic rage" into creative political purpose. The masses became for him the "self object" that provided the "emphathic mirroring" and self-esteem that had previously eluded him. This "psychic self-repair" enabled Gandhi to become one of history's most revered and effective leaders, but his own narcissistic needs and his longing for the "perfect Guru" remained unfulfilled.

In his rewarding essay, Bongiorno discusses the youthful Wilson's relationship with his parents and enriches Alexander L. George and Juliette L. George's pioneering study. He finds that not only the son but also the father was overly dependent, for both were narcissistically troubled. For example, Wilson Sr. wrote in a letter, "You are my alter ego; what pleases you equally pleases me . . . my heart cries out for you to an extent that can hardly be endured." The prolonged idealization of his father impeded the son's growth, but it also supplied him with the empathic mirror that gave him, as he said, the "spiritual capital" he so desperately needed.

Regarding Wilson's alleged strokes at Princeton, Bongiorno makes the point that, whatever the reality, Wilson believed that he suffered from the same disease that had killed his father and became convinced that he faced imminent psychic and physical "disintegration." During this crisis he confronted failure and despair—as he would later during the League controversy—exactly as his father had done: with inflexible, self-righteous, and self-defeating defiance. The tragedy of Wilson was that, in trying to find cohesion and order in his inner world, he resorted to means that impaired his brilliant capacities for academic and political leadership.

Thomas Kohut's carefully crafted and beautifully written essay argues that Wilhelm II's leadership was at the same time "dynamically compelling, yet weak and ineffective" (p. 180). Kohut explains this paradox by distinguishing Wilhelm's remarkable capabilities for symbolic leadership from his incapacity for political leadership. Kohut's thesis is that the Kaiser's "narcissistic psychopathology is at the heart of his historical significance" (p. 186).

A compelling need to overcome self-doubt and feelings of incohesion drove the Kaiser to act out the role of mighty, infallible leader. His compensatory exhibitionism, brilliantly staged public displays, and grandiose affirmations of power were warmly acclaimed by the new nation, which was itself disjointed regionally, socially, religiously, and intellectually. Thus, there was a remarkable psychological fit between ruler and nation: the nation served the Kaiser as an empathic self object; the Kaiser served the nation as its dynamic symbolic leader who was, in truth, Germany's "mirror image."

Although Wilhelm's mercurial instability and his "versatile theatricity" helped him play many different symbolic roles with striking success, these traits were a political liability, especially because this leader had no clear sense of political purpose. Incapable of charting or staying on a course, he steered erratically "full speed ahead" toward different objectives, interfered capriciously in the affairs of government, and indulged in irresponsible and belligerant verbosity—all of which contributed directly to his political failures in foreign and domestic affairs. In Kohut's telling summary: "The Kaiser personified the German nation not only through his effective symbolic leadership, but also by virtue of his very political incompetence" (p. 222).

This imaginative and controlled essay written by a person trained in both psychoanalysis and history demonstrates how much a psychologically informed historian can contribute to our understanding of the past. His model study, along with those on Gandhi and Wilson, would brighten any collection of essays on leadership; the dismal setting provided by the other articles in this volume makes them incandescent.

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PHILIP POMPER. *The Structure of Mind in History: Five Major Figures in Psychohistory*. New York: Columbia University Press. 1985. Pp. xvi, 192. $20.00.

EDWIN R. WALLACE IV. *Historiography and Causation in Psychoanalysis: An Essay on Psychoanalytic and Historical Epistemology*. Hillsdale, N.J.: Analytic; distributed by Lawrence Erlbaum, Hillsdale, N.J. 1985. Pp. xv, 285. $29.95.

Philip Pomper, whose earlier books were on Russian history, addresses himself here impressively to the psychohistorical conceptions of Sigmund Freud, Erik Erikson, Herbert Marcuse, Norman O. Brown, and Robert Jay Lifton. In order to link these disparate writers in a coherent text, Pomper devised a particular frame of reference that, for reasons of space, I cannot dwell on except to say that it is imaginative, intelligent, and instructive and that it allows Pomper to discuss these often formidable and complex writers in a lucid and economical way, making them much more accessible to readers than they have hitherto been.

The story begins with Freud and his belief that he could explain, on the basis of phylogenetic propositions, both the source of the contents that serve to organize behavior and the process by which these contents become effective. Freud's phylogenetic

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propositions obviously had to be abandoned, and the link between contents and process, which had carried him so far and of which he remained so confident, was broken. As a result, different writers interested in psychoanalysis have since been trying to reestablish this link, for the most part in a variety of psychosocial terms that encompass the subjective intentions and purposes of people. The reason is that explaining or interpreting the effectiveness of particular organizing contents without explaining the process by which they become effective must yield multiple, competing, and even contradictory interpretations of any but the simplest subject, with no grounds other than moral or ideological ones for choosing among them.

Some language theorists, critics, and historians argue that no solution to the problem of multiple, competing, and contradictory interpretations is possible, meaning in effect that no social psychology can adequately solve the problems presented by human subjectivity. One can also argue, however, that it is premature to foreclose on the possibility; that we do not yet have an adequate social psychology does not mean that we can never have one.

These issues of interpretation and theory are complex, but it is necessary to raise them to establish a context in which to view the other writers Pomper discusses. Because they too have not been able to solve the problem, their work can be examined as readily at the moral as at the theoretical level, and it is at the moral level that Pomper most tellingly deals with their work. Thus, Erikson is described as carrying on "Freud's task of enlightenment and universalism in a different, less scientistic and more religious voice, which proposes a psychoanalytic Golden Rule in the concept of mutuality" (p. 85). And Marcuse is described as showing "an almost desperate inventiveness" in order to find in psychoanalysis some basis for the legitimation of his particular utopian vision (p. 125).

By emphasizing the moral side of these writers' psychohistorical conceptions, at times focusing on only one of their works, Pomper is able to present his subjects as internally consistent and mostly discusses them apart from various inconsistencies and contradictions that occur in the body of work taken as a whole. Pomper is an exceptionally knowledgeable author, but, considering that these writers continue to be read for their theoretical contributions and that at least some of them have affected social practice in the past, he might have said more about the inconsistent and contradictory features of their work than he did.

Edwin R. Wallace IV also addresses problems in psychoanalysis and history, but in very different terms from Pomper. Wallace's concern is the epistemological status of psychoanalysis, his audience is the community of psychodynamic clinicians, and his task is to find a middle way between Freud's "excessively mechanistic conception of science," his "fallacious and superfluous" Lamarckian and biogenetic determinism (pp. 150–51), and the views of various critics (including psychoanalytic critics) who have argued that Freud should never have attempted to construct a general theory of a scientific character or to have thought about human motives and behavior in causal terms. Wallace is particularly offended by the claim of such critics that psychoanalysts do not so much help reconstruct a patient's past as help a patient construct a version of the past he or she can live with and that widely divergent interpretations have potential therapeutic value apart from any considerations of correctness (pp. 77–79). Wallace tries to develop some terms for a middle way (pp. 151, 162–63) in order to sustain the notion of psychoanalysis as a science (though not a mechanistic one) and to bypass this psychoanalytic version of "interpretive nihilism" (p. 82), organizing his discussion, specifically intended to be of interest also to historians, around various dichotomies such as positivism and historicism, idealism and realism, art and science.

Wallace, however, appears much too sanguine about the "evidential and inferential canons of psychoanalysis," which he claims are sufficient, along with "the configuration of the data" (p. 82), to ensure that there are objective grounds for psychoanalysts to choose from among possible interpretations. More important, he does not demonstrate that the problems of contents and process in history are solvable in psychoanalytic terms; on the contrary, his emphasis on genetic and energic propositions (pp. ix, 174) guarantees that they are not, because all dynamic solutions are then idiosyncratic by definition (p. 197) and provide no real sense of how social order occurs or how any such order emerged historically over time.

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JEAN STENGERS and ANNE VAN NECK. *Histoire d'une grande peur: La masturbation*. Brussells: Editions de l'Université de Bruxelles. 1984. Pp. 232. 750 F.

As moral device and as explanation for the otherwise inexplicable, masturbation has provided an extremely useful social mechanism in Western thought. This book recounts the development of ideas about masturbation from the eighteenth century to the present.

Traditional Christian thought considered masturbation as a sin "against nature" and against biblical judgment regarding the spilling of semen (Gen. 38, Cor. 6). Individual theologians might have railed

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against masturbation, but, as Jean Stengers and Anne Van Neck argue, from the fifteenth century onward concern with masturbation declined among their pastoral colleagues. Before the eighteenth century, secular authorities, medical writers, and educators treated masturbation with similar "indifference" (p. 45).

All changed with the appearance of two of the most influential, and wrong-headed, publications imaginable: in 1715 in London, the anonymous brochure *Onanism* and, forty-five years later, the Swiss physician Samuel-August Tissot's more systematic, "scientific" *Onanisme*. Masturbation, they argued, caused everything from amnesia and insomnia to scabs, gonorrhea, hemorrhoids, tumors, suffocation, and death. The two works wrenched discussions of masturbation from the theologian's exclusive domain and converted it into a medical problem. Treatment included special clothing, diet, exercise, surgery, and, above all, a campaign of unrelenting moral and psychological terror against those, male and female, who engaged in masturbation.

In Germany, the campaign against masturbation took on nationalist tones; in France, "one would look in vain for a difference between Conservatives and Republicans, between Catholics and free thinkers" (p. 16). Remarkably, universal acceptance of these ideas occurred without active encouragement from civil, academic, or even church sources. Not until 1875–81, when James Paget in England and Charles Mauriac and Jules Christian in France publicly repudiated Tissot's notions on the relationship of mental illness to masturbation, did it become possible to challenge the orthodoxy of antimasturbationism.

By the 1920s, in spite of the conservative opinions of people as disparate as, for example, Sigmund Freud and Robert Baden-Powell, masturbation was increasingly being considered not only as normal but also as having positive psychological and physical benefits. Stengers and Van Neck bring the story to the present, via the 1948 Kinsey survey (92 percent of adult males masturbate to orgasm) and the Roman Catholic Church's 1983 statement maintaining masturbation as "a serious moral disorder" but nonetheless calling for prudence and understanding in its "treatment." They cite contemporary sexologists like Wardell B. Pomeroy and Sherry Hite who advocate masturbation as part of an individual's right to pleasure. But other recent sources have escaped Van Neck and Stengers, such as best sellers like *Portnoy's Complaint* (which, it has been said, does for masturbation what *Moby Dick* did for whales) and the important discussions on autoeroticism taking place in contemporary gay and lesbian circles.

The book's major shortcoming is its failure to discuss the fear of masturbation within the larger context of nonprocreative sexuality in general. Homosexuality, for example, was long considered a sickness, a form of social revolt, and a natural conduit to suicide, for which (like masturbation) heterosexual marriage was believed to be a sovereign remedy. Michel Foucault's insights on the history of sexuality, on medical and social categorization, would be particularly relevant here: how does the discussion of masturbation illuminate the broader problem of deviance in recent Western culture? Admittedly, Stengers and Van Neck chose to write an intellectual rather than a social history of masturbation, but, in dismissing the impact of such issues as urbanism, family life, and technological change, we are left with only part of the story.

Van Neck, a student and colleague of Stengers at the Université de Bruxelles, died in 1982 as they were collaborating on this study. As Stengers tells us in his introduction (p. 8), this book might have been considerably more complete had she lived. Its contribution, and my criticisms of it, must be understood within that context.

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DIETER RULOFF. *Geschichtsforschung und Sozialwissenschaft: Eine vergleichende Untersuchung zur Wissenschafts- und Forschungskonzeption in Historie und Politologie.* (Forschungsergebnisse bei Oldenbourg.) Munich: R. Oldenbourg. 1984. Pp. x, 467.

This book is without question the most systematic and comprehensive critical examination of major orientations in contemporary historical theory as they relate to the practice of the historian. Dieter Ruloff examines the broad spectrum of writings from hermeneutics to cliometrics. He challenges hermeneuticists (Hans-Georg Gadamer, Jürgen Habermas, and also Jörn Rüsen) because, he believes, the contexts that interest historians and social scientists generally are "too complex to permit us to rely on intuition or introspection" (p. 43). He accepts the empirical analytical conception of scientific explanation but recognizes its limits. In his opinion the ideal form of explanation in history, as in all sciences, involves theory. Computer simulation thus has a valuable place in historical research. But theory, because of its abstract character, seldom does justice to the meaning and context found in history. A form of scientific explanation must therefore be found that not only involves the use of concepts but also respects these elements of meaning and uniqueness. Ruloff sees this middle way in models, which he distinguishes from theories. Theories are deductive: they systematically subordinate hypotheses to a central concept. In contrast, models use multiple hypotheses that generally cannot be systematized or

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validated but that provide a device for increasing knowledge of a real historical subject. This approach is basically pragmatic. There is no single way to historical knowledge. Ruloff agrees with Arthur Danto that narrative, too, can contain explanation and is capable of analyzing structures, as he shows in the analysis of Golo Mann's narration of the revolutions of 1848–49. Even literary evidence can be subjected to computerized content analysis, which can serve as a useful check on impressionistic causal explanations without constituting a substitute for the historian's critical understanding of contexts and meanings.

Ruloff questions whether the West German school of "historical social science," specifically Hans-Ulrich Wehler, uses theories in its analysis of the modern social history of politics. Wehler, he argues, does not understand what is meant by theory and in fact loosely borrows isolated social science concepts to buttress an essentially narrative presentation. This may be the case, but it seems to me that Ruloff is not that far removed from Wehler in his conception of what constitutes historical science. Like Wehler, Ruloff is basically committed to what he considers a scientific or a social scientific approach that recognizes the elusiveness of the historical subject matter. He, too, emphatically rejects the intuitionism of classical historical studies and stresses the need of method and conceptualization while, like Wehler, eschewing a narrow scientification of history for a pluralism of methods extending from cliometrics to thoughtful narration. The result is close to the theoretical and methodological eclecticism of which he accuses Wehler, an eclecticism that, in his view, should be justified only insofar as it furthers historical understanding. What distinguishes him from Wehler is his rejection of a critical political note.

Despite its comprehensive scope, this study contains several serious gaps. Thus, Ruloff does not examine the important French structuralist and deconstructionist discussions as they relate to history. His examination of French thought restricts itself to a very brief section on the Annales school derived primarily from a polemical article by Dieter Groh. More serious, Ruloff neglects the entire American and French narrativist discussion of the last twenty years, which has gone into directions much more hostile to method and explanation than Danto's *Analytical Philosophy of History* (1965)—a book that Ruloff still treats as the last word on narrative theory. Ruloff makes some interesting observations on Marxist discussions, which he finds in many ways parallel to Western ones on the problems historical reality poses for theory, in this case historical materialism. But he restricts himself to East German writings (by Peter Bollhagen) that are now, too, almost twenty years old.

Nevertheless, this is a useful and thoughtful work that deserves to appear in English. The English version, however, should not be a translation but a more tightly argued revision that should also address the serious challenge of narrativist and linguistic approaches to traditional conceptions of social science history.

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DOMINICK LACAPRA. *History and Criticism.* Ithaca: Cornell University Press. 1985. Pp. 145. $17.50.

At the beginning of one chapter Dominick LaCapra cites Montaigne: "There is more to-do about interpreting interpretations than about interpreting things" (p. 95). This book has more to do with interpreting interpretations than with interpreting things. That is proper, for historical "things" do not exist separately from some sort of interpretation, and it is the business of students of historiography to give attention to interpreting interpretations. In this, LaCapra is very good. His critiques of Carlo Ginzburg's *The Cheese and the Worms* (1980), Carl Schorske's *Fin-de-Siècle Vienna* (1980), and H. Stuart Hughes's *Consciousness and Society* (1958) make clear why each of these important works leaves the reader uneasy about contradictions in approaches and conclusions. LaCapra believes there should be closer ties between history and literary criticism, and these specific analyses provide a good example of the value of such a rapprochement. Where the book provides less than it might is in the creation of theory. LaCapra's attempt to define a "dialogic" interchange or "transferential" relationship between historian and the past is never articulated in any systematic way, nor is his research program in intellectual history. The author's talent is expended more in verbal display and flashes of brilliance than in the systematic theory building of which he is clearly capable. Perhaps a limitation of contemporary literary criticism and its practioners is that being spectacular is as important as being substantive. Although that generates excitement in the field, it does not help much in advancing the kind of theory needed by historians.

Perhaps the author sees that as beneath him. In another context he writes: "Indeed the key function of epigones would seem to be to reduce the problematic texts of 'initiators of discursive practices' (in Foucault's phrase) to their generic or paradigmatic level, thereby making them more adaptable to certain institutional uses. The more challenging and, at times, disconcerting text seems to rewrite the genre or take part in a continual founding and altering of expectations" (pp. 140–41). This reviewer hopes

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that there are epigones at hand to take up the task of adapting LaCapra's work to broader usage. LaCapra is engaged in a running battle with institutions, particularly that of the "orthodox" historical profession, which makes him especially sensitive to the relationship between institutions and a particular kind of freedom. "One thing an institution should be is a setting for dialogic encounter in which limiting norms necessary for life in common are put to tests that may strengthen or transform them. Indeed a humanistic discipline remains vital to the extent it is possible within it to engage points of view that pose fundamental questions to one's own. The difficulty is to create the material and intellectual conditions in which such an exchange is actually possible" (p. 142). The elaboration of that idea is much too important to be left to epigones.

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CHARLES TILLY. *Big Structures, Large Processes, Huge Comparisons*. New York: Russell Sage Foundation; distributed by Basic Books, New York. 1984. Pp. xii, 176. $14.50.

Charles Tilly is a member in good standing in academic history and sociology. For two decades he has contributed significantly to the "new social history" (deciphering the messages of inarticulate masses in strikes and riots) and to "historical sociology" (moving sociological theories of collective action and resource mobilization down to historically testable levels). In 1982 the measurement-oriented Russell Sage Foundation invited him to help celebrate its seventy-fifth anniversary by writing down some personal reflections on strategies for the analysis of social change. Although not yet an elder statesman nor moved to compose an *apologia pro vita sua*, Tilly responded with an uplifting essay that is meant to reassure readers of the good prospects for harmonious relations between social scientists and historians. As head of an institutional apparatus of his own (first at the University of Michigan and now at the New School for Social Research), he seems strongly motivated to take an optimistic line that can encourage the next generation of researchers.

As we approach the twenty-first century, Tilly argues, it is high time to get rid of all vestiges of the vast ahistorical and evolutionary schemes from the nineteenth century, especially with regard to the nation-state and capitalism. He attacks eight "pernicious postulates" that have (mis)guided many modernization studies: (1) there are distinct societies, (2) social behavior results from individual mental events, (3) social change is a coherent social phenomenon, (4) standard stages succeed one another, (5) differentiation leads to advancement as well as to (6) disorder, (7) disorder results from rapid social change, and (8) distinct processes of order and disorder lead to varieties of legitimate and illegitimate forms of conflict. Tilly devotes two chapters to illustrating the ways in which this "nineteenth-century folk wisdom" has remained influential in recent work in social science (p. 13). He is especially critical of the combination of these old-fashioned theories with statistics and judges, correctly in my view, that "little of long-term value to the social sciences has emerged from the hundreds of studies conducted during the last few decades that have run statistical analyses including most of the world's national states" (p. 77).

Tilly's antidote will sound unexceptional to many historians. He pleads for "historically grounded analyses of big structures and large processes" (p. 2). These analyses have taken place on four levels: world-historical, world-systemic, macrohistorical, and microhistorical. After establishing a polite distance from the levels on which Fernand Braudel and Emmanuel Wallerstein operate and after giving microhistorical knowledge its due, Tilly states his case for "huge (but not stupendous) comparisons" (p. 74) on the macrohistorical level. He distinguishes four basic comparative strategies—individualizing, universalizing, variation-finding, and encompassing—and exemplifies them with the works of Reinhard Bendix, Theda Skocpol, Barrington Moore, and the late, lamented Stein Rokkan, for whom he feels particular affection. All four strategies have their uses, but Tilly makes a special pitch for the encompassing comparison because of its twin advantages in "directly taking account of the interconnectedness of ostensibly separate experiences and [in] providing a strong incentive to ground analyses explicitly in the historical contexts of the structures and processes they include" (p. 147). His main message to historians who reject the ahistorical and developmental traditions is a plea not to abandon the comparative method but to root it in historical concreteness and to recognize that they will only profit from the advance of a truly historically grounded, comparative social science.

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SUSAN GROAG BELL and KAREN M. OFFEN, editors. *Women, the Family, and Freedom: The Debate in Documents*. Volume 1, *1750–1880*; volume 2, *1880–1950*. Stanford: Stanford University Press. 1983. Pp. xiv, 561; xiii, 474. $14.95; $13.95.

ERNA OLAFSON HELLERSTEIN *et al.*, editors. *Victorian Women: A Documentary Account of Women's Lives in Nineteenth-Century England, France, and the United States*. Stanford: Stanford University Press. 1981. Pp. xvi, 534.

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Let it be said at the outset that the three books under review will be of considerable interest to social and intellectual historians as well as to those whose specialty is women's history. This is no mean accomplishment.

The two-volume collection, *Women, the Family, and Freedom*, presents the clash of ideas surrounding the woman question and includes the issues of education, employment, marriage, motherhood, and civil and political rights. This debate, which began *sotto voce* in the eighteenth century, crescendoed in the nineteenth century. Susan Groag Bell and Karen M. Offen make the valid point that the explosiveness of the woman question in its social, political, and economic ramifications has thus far been underestimated. The editors' use of wide-ranging sources—American, British, French, Italian, Scandinavian, and Russian—highlights the cosmopolitan character of the debate. The juxtaposition of the documents according to prevailing and dissenting viewpoints often reveals bitterness and acrimony, especially of unrelenting male chauvinists like August Strindberg and Friedrich Nietzsche. Although the editors have chosen their examples judiciously, this approach makes it appear that the dissenters carried equal weight with the traditionalists, which is somewhat misleading.

Bell and Offen should be commended for their inclusion of literary figures such as Charlotte Bronte, Henrik Ibsen, and Olive Schreiner. Because women were avid readers of fiction, novelists and dramatists often had a greater impact on public opinion than philosophers, politicians, and social scientists. The editors' choices also reveal that women were often fervent supporters of tradition, whereas a number of men argued vehemently for women's liberation from the domestic sphere to which women were consigned by tradition.

In *Victorian Women* Erna Olafson Hellerstein *et al.* use women's diaries, letters, medical and legal records, wills—the main sources for social history—to give an overview of women's lives from the cradle to the grave. Women relate their experiences starting with the uncertainties of girlhood, progressing to the conflicts surrounding work and family in adulthood, and ending with the tribulations of old age.

All the books have general introductions as well as concise and informative introductions to each section. A summation of each document places it in context and provides a useful frame of reference.

Every reader is bound to find flaws in works of this scope and magnitude. Some will be distressed by the omission of certain individuals from the debate in the Bell and Offen volumes. In addition, historians of woman's history have become increasingly sensitive to the categories that are used to organize historical materials. It has become almost a truism in woman's history that the historical watersheds by which males measure their progress in attaining political and civil rights do not have the same relevance for women. Only by treating the eighteenth-century (male-defined) democratic revolutions as a unity can it be made clear that, when the dust had settled, white middle-class males had made significant gains at the expense of women of all classes. Judith Sargent Murray, one American woman who realized this, is unaccountably tucked away with two prerevolutionary female critics of male authority. Is "the Romantic era" a meaningful umbrella for the highly charged debates about women's roles between 1830 and 1848? As for the revolutions of 1848, neither liberals nor radicals, much less the triumphant conservatives, had any change in the legal status of women on their agenda.

One would wish for the inclusion of those events that touched women sufficiently to spur their involvement in public affairs. The long campaign the women of England waged for the repeal of the Contagious Diseases Acts would qualify. These acts, which made women scapegoats for the epidemic of venereal disease raging in the British army, brought about an unprecedented unity of women of all classes in England. The ultimate success of the repealers in 1886 strengthened the case for women's freedom immeasurably and profoundly impressed individuals as diverse as Eleanor Marx and John Ruskin.

One error in volume 2 of *Women, the Family, and Freedom* must be noted. The selection from G. B. Shaw's *The Quintessence of Ibsenism* (p. 41) is evidently from the 1913 edition and not from the original (1891), which is cited. The tip-off is that Shaw refers to the "late" William Stead in his critique of Stead's attitude to the "unwomanly woman." Stead, the crusading journalist who exposed the prevalence of white slavery, went down with the Titanic in 1912. He was very much alive in 1891.

Victorian Women covers the United States, England, and France. To justify the inclusion of France, to which the term "Victorian" seems misapplied, the editors argue that all three nations compose a transatlantic community and that all three were marked by instability during this period. The triad did indeed constitute a transatlantic community in the eighteenth century—all were influenced by the philosophes of the Enlightenment—but not in the nineteenth century. The instability to which the editors refer was characteristic only of France, beset as it was by revolutions, coups, and "men on horseback." French women, whose tactics were timorous, did not form organizations until the 1870s, a generation later than American and English women. Whereas American and English women received the vote almost simultaneously (1920 and 1918 respectively), French women had to wait until

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1945 for this basic right. Thus, a case can certainly be made for treating French women separately.

It is clear from the varied sources in *Victorian Women* that nineteenth-century women had more choices regarding marriage (whom to marry and whether to marry) than was previously thought. They seemed to be aware that they were in the eye of a storm of debate about their proper roles. The zealousness with which they recorded their feelings and experiences suggests healthy egos and a sure sense of their own importance.

Despite revisionist assaults on the stereotype of the average Victorian women, such as her "passionlessness," she still emerges as God-fearing and earnest; even in dire circumstances, she never doubts the omnipresence of a merciful God. And there is not a woman who does not take herself with the utmost seriousness. It is to the credit of the editors that they respect these traits, which constitute an unbridgeable gulf between the Victorian and the modern woman, and do not try to gloss over them in the name of continuity.

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SIDNEY W. MINTZ. *Sweetness and Power: The Place of Sugar in Modern History*. (Elisabeth Sifton.) New York: Viking. 1985. Pp. xxx, 274. $20.00.

West Europeans planted two long-enduring economic institutions in the Caribbean and Latin America: silver mines in Mexico and Peru and sugar estates on the islands of the "West" Indies and the mainland of Guiana and Brazil. The plantations, more than the mines, have attracted scholars, perhaps because the former continued to generate wealth and income for their proprietors while maintaining exploitation of the labor force. Sugar and slaves, then sugar and wage labor, have left a bitter legacy in plantation America.

Plantation studies have spotlighted myriad facets of the Caribbean economy, especially those on the English islands, leaving in a penumbra their impact on Europe's economy, society, and culture. A notable exception is Eric Williams's *Capitalism and Slavery* (1944), a pioneer analysis of how the plantation "financed the Industrial Revolution in England" and the intensification of "British capitalism." Williams, like Fernando Ortíz (who saw sugar as the "favored child of capitalism"), foreshadowed the broad view of Latin America in the Atlantic economy that economist Raúl Prebisch encapsulated as the link between the center and periphery and that André Gunder Frank and Immanuel Wallerstein put into a longer historical perspective of development and underdevelopment. More recently, Eric Wolf, whose *Sons of the Shaking Earth* (1959) remains a classic cultural history of Mexico, has assessed the core-periphery, or metropolitan-colonial, relationship in *Europe and the People without History* (1982).

Sidney W. Mintz's intellectual trajectory parallels that of Wolf. Mintz first researched the New World plantation complex in a study of a Puerto Rican estate ("Cañamelar"), followed by a cane-worker's biography and a perceptive introduction to the classic Caribbean study by Cuban historian Ramiro Guerra y Sánchez. Mintz now projects the impact of the plantation economy on the English metropolis, whose economy and society from 1650 to 1850 absorbed the output of its Caribbean sugar islands, becoming Europe's largest per capita consumer of sucrose.

Sweetness and Power traces the cultural history of a cultigen, sugar cane, from its first presumable domestication in New Guinea through its cultivation in India, the Mediterranean islands, the Andalusian coast, Morocco, and the Atlantic islands to its transplantation to Brazil and, ultimately, the Caribbean. In "Production," the first of three major chapters, Mintz argues that West Indian plantations, in organization, size of labor force, division of labor, and insistence on discipline, foreshadowed the factory of the later Industrial Revolution, an "unusual combination of agricultural and industrial forms . . . unlike anything known in Europe at the time" (pp. 47, 48). Yet on the capitalistic nature of the plantation Mintz is cautious; the West Indian estate, he concludes, "nourished certain capitalistic classes at home *as they were becoming more capitalistic*" (p. 66).

Mintz's primary focus is on sugar's commercialization in England and its social and economic role. Rare in 1650 and still a luxury in 1750, a century later sugar had become a necessity "in an expanding capitalist economy." Some factors in sugar's commercialization are obvious: the profits of production in the colonies and of transport and distribution in the metropolis, along with the sales of metropolitan goods to the colonies. Less obvious, as Mintz indicates, is the role of calorie-rich sugar in filling the caloric insufficiency of the English working-class diet. Its popularity in working-class households coincided with women's entry into the factory system and their diminished presence in the home. In tea and in jam spread over store-bought bread, sugar liberated "the wage-earning wife from one or even two meal preparations per day" while supplying calories (p. 130). In working-class family consumption patterns, Mintz notes that it was customary for the adult males to consume protein foods, leaving to women and children cheaper sucrose. He hypothesizes that children's protein insufficiency "may constitute a kind of culturally legitimized population control" (p. 149), while the high sucrose intake of rural as well as industrial labor "sharply reduced the

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over-all cost of creating and reproducing the metropolitan proletariat" (p. 180).

Implicit in these hypotheses is the presumed role of human agency, the "controllers of society"—planters, bankers, slavers, shippers, refiners, grocers, and bureaucrats—in transforming the English working-class diet. In "Power," Mintz offers two categories of analysis, an "inside" and an "outside" meaning of power, categories that are often blurred. By "outside" he means overt political lobbying favoring planters, slavery, the slave trade, and the availability of sugar, rum, and molasses in England; "webs of signification" offer the "inside" meaning, which may be interpreted as the internalization of codes of behavior and food habits. Mintz implies that, to increase sugar consumption, the sugar interests worked to monetize the English economy, pushing the lower classes into the marketplace, raising expectations, and thereby stimulating productivity. Thus, the English laborer found "new tasting opportunities and new occasions for eating and drinking" (p. 165) while absorbing "the complex idea that one could *become* different by *consuming* differently" (p. 185). Primitive consumerism transformed sugar into an "opiate," providing "symbolic demonstration that the system that produced it was successful" since the working class by emulating its betters in sugar consumption enjoyed the illusion of freedom of choice. In Mintz's optic, not the sugar lobby alone but the "context, or a set of situations, created by broad economic forces" extended sugar consumption in England (p. 181).

Mintz has produced a work of general as well as scholarly interest, both an introduction to the social history of a commodity and an "anthropology of modern life." In its rich texture lies the vision of what sugar tells us about colonialism and about preindustrial and industrializing England, as well as showing us the anthropologist's ecumenical perception of change and how the world "manages at the same time to stay in certain regards very much the same" (p. xxvii). By integrating colonial and metropolitan economies and suggesting we view the colonial plantation as a precocious prototype of industrial organization, this book reminds us of the unequal distribution of the gains of Europe's economic development, which accelerated with commodity production in the New World and the slave trade. Capitalism's legacy in the colonial periphery, and even in its metropolitan core, remains persistent poverty.

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RAYMOND F. BETTS. *Uncertain Dimensions: Western Overseas Empires in the Twentieth Century*. (Europe and the World in the Age of Expansion, number 10.) Minneapolis: University of Minnesota Press. 1985. Pp. xv, 263. Cloth $22.50, paper $11.95.

Uncertain Dimensions is an important contribution to the rather sparse literature dealing with the course of empire in those years of the twentieth century when the bloom was clearly off the imperial rose. Furthermore, Raymond F. Betts does not limit his treatment to the British or any other particular empire but is comparative in his approach. This makes the book particularly valuable, because the greatest lacuna in the area of imperial history is the dearth of good comparative studies.

After establishing the context for his work, Betts analyzes the effects of World War I on the various European empires with particular attention to the Middle East and Africa. Chapter 2 is devoted to an analysis of postwar imperial administration. Not surprisingly, the emphasis is on the French and British empires, but the discussion of Italo Balo and Italian policy in Libya is particularly revealing. The author notes that the post–World War I period was one of changing philosophical and psychological concepts toward the governed peoples. Gone were the destructive precepts of Social Darwinism. Cultural differences became the order of the day. "It was not so much the new in conflict with the old as it was the modern in opposition to the traditional that Europeans saw as the nature of this cultural contact" (p. 52). Based on this new attitude, the concept of trusteeship was born.

The financial ruins in which Europe found itself in 1919 forced politicians and statesmen to look to the colonies for assistance: hence, the inception of major programs of road and railway building and an emphasis on vocational education for the indigenous populations of the various empires. As early as 1895, Joseph Chamberlain described the British empire as an "underdeveloped estate" and commented that, "if the people of this country are not willing to invest some of their superfluous wealth in the development of their great estates, then I see no future for these countries" (p. 93). Nevertheless, the efforts made to use overseas empires to ameliorate the problems of the metropole were by and large failures.

Chapter 4 constitutes a fascinating excursion into the nature of colonial cities established by Europeans and the philosophy that inspired their design and construction. The book concludes with the rise of protest and rebellion in the colonies, the trauma of separation, and a brief treatment of post-partum events. A useful bibliographical essay concludes the volume.

All in all, Betts provides the reader with an elegant piece of work. Written with style and verve,

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it covers an important period of imperial history with authority, insight, and intelligence.

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WALTER A. MCDOUGALL. . . . *the Heavens and the Earth: A Political History of the Space Age.* New York: Basic Books. 1985. Pp. xviii, 555. $25.95.

Walter A. McDougall has filled a major gap in the historical literature by providing us with a sophisticated and comprehensive overview of the advent of the space age. His primary concern is with the political consequences of the technological revolution that began with the Soviet Sputnik and reached a climax with the American astronauts landing on the moon. Not content with merely chronicling these momentous developments, he offers a stinging critique of technocracy, blaming the politicians, not the scientists or engineers, for going overboard in embracing the new technology as the answer to all of mankind's problems.

The book's major contribution lies in its well-researched and clearly presented account of both American and Soviet space policy, although his sources enable him to paint a much fuller picture of the U.S. effort. Displaying a mastery of the existing body of literature on space developments, McDougall supplements this information with a great deal of original research in the NASA archives and the presidential libraries, especially in the Eisenhower and Johnson manuscript materials. The result is an in-depth study that explains why the Soviets were able to surprise both America and the world with the first earth satellite but eventually fell behind an aroused United States in the 1960s. The hero of his account is Dwight D. Eisenhower, who is portrayed as wisely preaching restraint despite appeals for an all-out American acceptance of technocracy, while Nikita Khrushchev is the villain, cynically using Sputnik and early Soviet space spectaculars to disguise the fact that the Russians were falling further and further behind the United States in the ensuing scientific and technical race. The author treats John F. Kennedy and Lyndon B. Johnson more neutrally, as victims of the delusion that technology could not only unlock the secrets of space but also enable governments to solve all their problems, from poverty and urban decay to education and civil rights.

McDougall is strongest on the impact of Sputnik, which he likens to Pearl Harbor, an analogy made by many in the late 1950s. The initial reaction was one of humiliation and despair, but the event awakened a sleeping giant and soon led to a vigorous and successful national effort to surpass the Soviets in missiles, satellites, and space adventure. The author traces the American failure to launch the first earth satellite to Eisenhower's insistence on a civilian effort divorced from the military's quest for a workable intercontinental missile. Intent on developing a reconnaissance satellite that could replace the U-2 as a way to monitor the Russian missile program and make arms control possible, Eisenhower saw Sputnik as legitimizing future American spy satellites. He was unprepared for the panicky public response, however, and finally was forced to undertake a more strenuous military and scientific effort in space than he thought desirable. Even so, Eisenhower succeeded in preventing a military monopoly of space with the creation of NASA and his insistence on a balanced American program.

The value of McDougall's synthesis of the dawn of the space age is damaged by his heavy-handed and intrusive moralizing. Convinced that Sputnik ushered in a new trend toward technocracy, he sees governments seeking not just to respond to technology but also to exploit and control the new scientific and technical advances for questionable national goals. As a result, McDougall's book, more a philosophical than a political history of the space age, raises as many questions as it answers. The last two chapters in particular, concerned with denouncing systems analysis and exploring the spiritual meaning of the new reliance on technology, seem especially out of place. Yet, despite these faults, his book offers a broad panorama of the opening decade of the space age; it is a study filled with insights and new revelations, as well as challenging interpretations that invite further scholarly investigation and debate.

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University of Texas

ANCIENT

IRVING M. ZEITLIN. *Ancient Judaism: Biblical Criticism from Max Weber to the Present.* Cambridge: Polity. 1984. Pp. xiii, 314. $24.95.

Irving M. Zeitlin promises to make a major contribution to the sociology of the religion of Israel in the tradition of Max Weber. The subject is timely, and the book is likely to provoke great interest among biblical scholars. The resulting study, however, is a complete disappointment.

The book begins with a brief review of Weber's sociology of ancient Judaism and gives a survey of Near Eastern polytheism as a foil for the religion of Israel. The author then moves from the time of the patriarchs through the various historical periods to the exile and restoration. His primary concern is to identify the social origins of ethical monotheism in

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the time of Moses and to limit the religious achievement of the prophets to the elimination of the persistent evil of fetishism. Zeitlin thus seeks to revise the views of Weber and to offer a critique of current biblical scholarship.

This book is a disappointment because it contains nothing new. Apart from occasionally using sociological jargon, the author does not usefully apply sociology to the study of ancient Israel. He seems unaware of, or ignores, the whole of the current application of sociology to biblical studies: the works of Norman Gottwald and Robert Wilson, for example, are never mentioned. Instead, Yehezkel Kaufmann's viewpoint dominates the book. The professed review of biblical scholarship is grossly inadequate. Scholars' words and views are used and abused in whatever way it suits the author's reconstruction. The diatribes against the "hypercritics" are only a pretense to scholarly debate. What always counts in the end is a literalistic reading of the biblical text.

The book is also full of mistakes of every kind. Names of scholars (including that of the reviewer) are misspelled or incorrectly rendered (Rad for von Rad and Vaux for de Vaux), and historical and geographical errors abound. In the rapidly changing fields of archaeology and Near Eastern studies, the author makes no attempt to be up to date. Surely Zeitlin could have avoided many of his errors by consulting some of his colleagues at Toronto.

The book is actually an apologetic treatise for a conservative religious position. For the general reader it is a highly misleading introduction to biblical criticism, and for the specialist it is a waste of time.

JOHN VAN SETERS
University of North Carolina

RUSSELL MEIGGS. *Trees and Timber in the Ancient Mediterranean World.* New York: Clarendon Press of Oxford University Press. 1982. Pp. xviii, 553. $79.00.

A prospective reader might well glance at this not inconsiderable work and decide that it likely tells a good deal more about trees and timber than one might reasonably want to know. And, indeed, few historians will wish to read it right through. The book is well done, however, and the topical organization will make it useful as a reference book, even though its scope is limited: it deals with the Near East in part, Greece, and Italy but leaves out the rest of the Mediterranean world.

Russell Meiggs writes, in a rather inappropriate metaphor, that he does not intend "to break new ground, but to help those who are even less familiar with trees and forests than I was when I started" (p. 5). Most of us belong in that category, for Meiggs served during the Second World War as an officer in charge of home timber production in Britain, which generated a lifetime of interest in the subject.

The author has carefully combed through the literary sources and made extensive use of archaeological and epigraphical data. On occasion, he broadens his scope to embrace related topics: a section on timber and ships expands into a discussion of the importance of naval power; another on the use of wood in architecture leads to a disquisition on architectural methods.

Meiggs attacks specific problems: were the cedars of Lebanon really cedars? Contrary to the views of some scholars today, he thinks they were (appendix 2 and elsewhere). Do the ancient terms always mean what they seem to mean? No. The Greek *kedros*/Latin *cedrus* refers not always to cedar but often to juniper or even other trees (appendix 3, "Confusion of Species"). Where did Athens get the timber to build her fleets? From Macedonia, usually, but not for the 200 triremes built by Themistocles when the Persians were in occupation of the critical areas. Timber then probably came from southern Italy (pp. 121–30).

Surprisingly, Meiggs holds that ancient armies used more timber than navies did. The point is valid, but surely most of the wood cut by armies—such as for firewood and temporary fortifications—must have been relatively small stuff as compared with the long beams of high quality that were required for shipbuilding. Even siege equipment and bridges would hardly have required the best timber.

The writer understands well the qualities of the various sorts of timber and their specific applications. Fir, for example, was frequently used in ship construction, not because it is the strongest wood but because it is strong and lightweight and comes in straight, long lengths. But the Phoenicians often used cedar on account of availability.

Meiggs also knows the uses of timber in the medieval and the modern worlds. He has, for example, investigated the modern stands of timber varieties important in antiquity. He remarks, somewhat irrelevantly, that one reason why the British had trouble with their fleets during the American Revolution was the loss of the New England white pine, which furnished the best masts of the day.

There are nine appendixes, a special and useful index of trees and their woods, an extensive bibliography, and several illustrations. One small lapse: in appendix 9, "Measures, Coins, and Weights," the minae is listed at 60 drachmae. In the most common Greek standard, the Attic, the correct figure is 100. Thus, the Attic talent contained 6,000 drachmae, not the 3,600 one would derive from the information here.

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The book is both interesting and useful. Students of Greco-Roman antiquity and those interested in the eastern Mediterranean littoral will find it profitable at least to examine it; most will find sections that will intrigue them.

HENRY C. BOREN
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K. H. WATERS. *Herodotos the Historian: His Problems, Methods, and Originality*. Norman: University of Oklahoma Press. 1985. Pp. 194. $19.50.

Intended to introduce college students without a knowledge of Greek to Herodotus, K. H. Waters's latest study of the Father of History will be welcomed by their instructors as well. In twelve concise but by no means cursory chapters, Waters first locates Herodotus in the Greek intellectual and historical milieu, then examines seven important aspects of Herodotus's work, and concludes with two chapters of evaluation. The text reflects Waters's views on a large number of controversial questions, the notes concentrate on current historiography, and the bibliography includes most recent work.

Both beginning and advanced students can profit from this book. Happily, beginners will not be able to use it to avoid working with Herodotus, and even those reasonably familiar with Herodotus will need a copy of the text close by to look up the passages to which Waters refers as he marshals the evidence for his interpretations.

Waters considers Herodotus a curious, cosmopolitan, wry, and usually shrewd reporter who sought not only to inform but also to entertain a wide audience. To both tasks he brought moral and political objectivity. Waters fashions a sympathetic but never sentimental portrait of Herodotus by treating Herodotus's selection of subject matter, the structure of his history, his narrative techniques, his sources of information, his religious and moral attitudes as well as his prejudices, and his characterization of individuals. Throughout his discussion, Waters takes pains to describe from two vantage points the problems Herodotus confronted: that of the mid- to late fifth century in general and that of the mind of a man like Herodotus in particular. This double focus allows Waters to show how Herodotus transcended the intellectual climate and popular morality of his own era to produce a work that celebrated barbarian as well as Greek achievements.

Of particular value is the chapter on Herodotus's sources of information. Waters emphasizes here, and elsewhere, how dependent Herodotus was on oral tradition and the skeptical stance he took toward such information. Where Herodotus could investigate further, he often did; when he could reconcile different accounts, he tried to do so; when he had no basis for evaluation, he simply reported what was said.

Although this book reveals both enthusiasm for and admiration of Herodotus, Waters does not gloss over questions about Herodotus's shortcomings. The chapter "Herodotean Prejudices" refutes (usually with some success) most of the ancient and modern charges of bias (philobarbarism, partisanship for Athens and the Alcmeonids, anti-Ionian sentiment, misogyny). The chapter "Strengths and Weaknesses" forthrightly acknowledges the serious defects in Herodotus's military accounts; the problems with inconsistency, chronology, and geography Waters sensibly explains as the inevitable consequences of the scale on which Herodotus worked.

Herodotean studies will benefit from this succinct statement of Waters's thinking that builds on two decades of work in the field. Were the book issued in paper at a reasonable price, more students could be asked to add it to their libraries.

VALERIE FRENCH
American University

STEPHEN L. DYSON. *The Creation of the Roman Frontier*. Princeton: Princeton University Press. 1985. Pp. xii, 324.

The study of the definition, military organization, and societal impact of Roman frontiers has attracted so much scholarly interest over the last century that *limes* research is almost a discipline in itself. Until quite recently, however, it was the fully demarcated and very stable post-Flavian frontiers that monopolized attention. Visible remnants of the empirewide frontier system were sufficiently impressive to attract antiquarian interest from early days, and, when systematic archaeology began, it was made uniquely productive because of the extensive standardization of Roman military engineering, which allows the fully reliable definition of complete structures from fractional remains. Because of their sustained stability, moreover, the frontiers have yielded abundant epigraphic and numismatic evidence from the debris left behind by generations of troops on frontier duty. As a result, the various forms of walls, palisades, trench lines, and patrol roads, with their watchtowers, auxiliary forts, and legionary bases and their evolving deployments of troops and riverine fleets, have been clearly reconstructed.

But Roman frontier strategy long predated the precise demarcation of the post-Flavian era, and there was always much more to it than military engineering. The extension and defense of the frontiers involved the control of trade, more or less

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purposeful cultural diffusion, and diplomacy at many levels, whose tools included both subsidization and social penetration—with the whole moving array forming an invisible predecessor and complement to the military frontier as such. The concepts and procedures that guided the management of these invisible frontiers throughout the Principate and after had evolved from the earliest days of Republican expansion, as countless studies of particular localities and particular moments of Roman-native interaction have incidentally shown.

Stephen L. Dyson's book is the first attempt to present the evidence to that effect in a systematic and comprehensive fashion. As Dyson points out, even the Republic, whose external policy was shaped by the interplay of rival elite factions, had to have a basic *modus operandi* developed over time to deal with the peoples, problems, and opportunities successively encountered at the advancing periphery. And that is the focus of Dyson's survey of the successive phases of the extension of Roman control in the West, from Italy itself to Gaul, the Iberian peninsula, Sardinia, and Corsica, as well as Liguria.

Each of these areas is the subject of a separate chapter, which begins by depicting the *status quo ante* and attempts to reconstruct the area's pre-Roman history, beginning with the Iron Age in some cases. Dyson offers a synthesis of the great mass of new evidence now available, which complements the scant narrative information. For the nonspecialist, this preliminary to Dyson's main purpose suffices to justify the book.

Dyson must rely largely on the narrative sources to describe events and define evolving methods after the arrival of the Romans, because epigraphic evidence does not become significant until Romanization is well advanced. Overcoming the systematic defects of Florus's works and the equally systematic *Tendenz* of Livy's, Dyson's use of narrative information is highly productive and duly cautious; he construes a plausibly detailed account while being careful not to overconstrue, especially in regard to clientistic interpretations. Some doubts, however, will be aroused by Dyson's attempts to invoke parallels with other frontier and internal security experiences. For example, Lusitanian affairs in the second century B.C. and the career of Viriathus are not better explained by invoking E. J. Hobsbawm's banditry theory (pp. 208–09) whereby bandits exist on the margins of society (true, but obvious) and are either socially conscious (in the Robin Hood style) or not socially conscious (when they rob peasants as well). Actually, rural depredators can only be of peasant stock, and whom they rob will depend on who has something to rob, the wealthy if they exist locally or the poor if not—and neither haiduks nor any other depredators can usefully be categorized as "social bandits" or nonsocial bandits. That the imposition of Roman order, more intrusive and alien to boot, was also resisted by those who were already opposed to the non-Roman order is too obvious a phenomenon to require theoretical explanation.

One other criticism must be mentioned. The reading of Dyson's book is greatly complicated by the lack of maps of appropriate scale and type. This common defect is of uncommonly severe effect in a work largely focused on the evolution of political geography over time, and it is especially felt in the chapter on Liguria, whose history was so largely conditioned by topography. But the need to read Dyson's book with a competent historical atlas alongside does not diminish the value of the work to all students of Roman history; indispensable for the Republican period, it is enlightening for the Principate as well.

EDWARD LUTTWAK
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RAYMOND VAN DAM. *Leadership and Community in Late Antique Gaul*. Berkeley and Los Angeles: University of California Press. 1985. Pp. xii, 350. $35.00.

Although the title correctly indicates the context of this book, readers will find that Raymond Van Dam provides more than just an analysis of communities in Gaul and their leadership at the local and national level. Here is a fine picture of Gaul in late Roman and early medieval times when everything was in flux, old values changed along with boundaries and loyalties, and the Middle Ages began to take shape.

The book is divided into four major sections. The first, "Local Authority and Central Administration," investigates the importance of the imperial presence, "the animating source," in the province, which is contrasted with the stable, local Gallic aristocracy. This leads to a discussion of the *Bagaudae*, who may have been not a hostile movement of bandits but rather a "reemergence of a substratum of local leadership" the ultimate aim of which was "to revive a Roman administration that was abandoning them" (p. 53).

The second part, "Christian Society in Fourth Century Gaul," discusses the emergence of Christian bishops as local men of authority and the social functions performed by Christian doctrine and liturgy. In this chapter the position of women is discussed (Christian communities did not quite know what to do with them because Christianity was a "Man's world" [pp. 75–76], followed by the problem of Manichaeism, "the evil antithesis of acceptable society" [p. 79]). This leads necessarily to the problem of the heresy of Priscillian, which is given a fine treatment and a new interpretation: in Priscillianism, "people articulated unacceptable aspects of their communities" (p. 108).

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Part 3 is entitled "The Assimilation of Christianity and Society." The main topic of this section is the powerful influence of Saint Martin of Tours, whose career demonstrates "how Christian communities and their leaders appropriated the ideology and relationships of secular society" (p. 116) as a result of which Christianity became more appealing to local aristocrats. These finely written pages on Saint Martin are among the best in the book. Other leading personalities discussed in this section include Germanus, Paulinus of Pella, and Sidonius of Clermont. During Sidonius's lifetime, increasing emphasis was placed on the cults of local martyrs and bishops.

Thus, the fourth, and longest, part deals with the cult of relics in sixth-century Merovingian Gaul, that is, with the world of Gregory of Tours (533–94), whose life and importance is ably analyzed. The last two chapters in this section are called "Sacred Space" and "Sacred Time." They deal with the cult and the church of Saint Martin (the latter was a "living symbol of Christian society" [p. 254]) and with liturgy (the ringing of bells enabled people "to synchronize with the divine rhythms of heaven and they also aroused harmony and conformity within communities" [p. 299]). The book closes with an epilogue that describes the fates of Paulinus of Nola and Ausonius.

I learned much from this book, and I enjoyed reading it so much that I am almost tempted to ignore a very bad and confusing printing error on page 77; unfortunately, a reviewer cannot do that. Apart from this minor slip, however, I highly recommend this book not only to patristic scholars and church historians but also to historians of the Middle Ages and the later Roman Empire. In recent years many fine books, such as John Gager's *Kingdom and Community* (1975) and Howard Clark Kee's *Miracle in the Early Christian World* (1983), have appeared from the school of Max Weber. The present book is an outstanding contribution in the field.

STEPHEN BENKO
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GERHARD ALBERT. *Goten in Konstantinopel: Untersuchungen zur oströmischen Geschichte um das Jahr 400 n. Chr.* (Studien zur Geschichte und Kultur des Altertums, new series, monographs, number 2.) Paderborn: Ferdinand Schöningh. 1984. Pp. 211. DM 44.

Gerhard Albert has written a specialized study of events in Constantinople in the final years of the fourth century and at the beginning of the fifth century, primarily in A.D. 399–400. His basic subject is the clashes and alliances between the Goths Gainas and Tribigild, the fall of the court chamberlain (*praepositus sacri cubiculi*) Eutropius, the rise of Anthemius and Synesius, and the suppression of the revolts of Tribigild and Gainas against the Byzantine government. Albert has discovered no new primary sources but attempts a critical reevaluation of known sources and modern scholarship. His book is part of what is becoming a major reevaluation of the events during the Theodosian dynasty and of the significance of that period. It also belongs to a new wave of intensified interest in early Germanic history and institutions.

Many of Albert's conclusions are prudent. Having conscientiously studied the sources and modern scholarship, Albert correctly criticizes the tendency to assume the existence of "pro-German" and "anti-German" parties in Constantinople, around which polarity all political events can allegedly be interpreted. His monograph should help lay that myth to rest, for he convincingly shows that the situation was much more complex. He also subtly analyzes whether Gainas's followers among the Goths should be classified as *buccellarii* or *foederati*, and he sensibly explains why the simple designation of them as *buccellarii* is inadequate. Albert attempts to understand groupings among the Goths and refuses to treat them as monolithic. His is a reasonable approach.

This is a respectable contribution to the historiography of the fourth century, but already it appears to be a little bit outdated. Despite Albert's best efforts, recent research by Alan Cameron and Timothy Barnes, which appeared after Albert's book was in press, has radically altered the prevailing historical understanding of factional groupings at the end of the fourth century. Cameron has provided an important reinterpretation of Synesius of Cyrene's writings, in particular, his *De Providentia*. Cameron and Albert have conflicting opinions concerning the date on which Caesarius left, perhaps involuntarily, his post as praetorian prefect and the date of Anthemius's assumption of that office. It will be interesting to see how Albert will handle Cameron's controversial thesis.

There are other criticisms. This is not a long book, but Albert could have made his points more succinctly, perhaps in the form of one or two long articles. In his concluding excursus on the praetorian prefecture in the East he makes some dubious assumptions about that office. The volume contains an index, which could be more comprehensive: it lacks sufficient attention to Greek and Latin words and many other important subjects, because it includes only names and source references. This book will nevertheless be of interest to scholars of late Roman, Byzantine, early medieval, ecclesiastical,

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and Germanic history, as well as to some scholars of late Greek and Latin literature.

WALTER EMIL KAEGI, JR.
University of Chicago

RICHARD N. FRYE. *The History of Ancient Iran.* (Handbuch der Altertumswissenschaft, third series, number 7.) Munich: C. H. Beck. 1984. Pp. xiii, 411.

Spanning a millennium and a half of recorded history down to the Arab invasions of the mid-seventh century A.D., this survey of the world of the ancient Iranians is a richly colored and crowded tapestry, across which Richard N. Frye weaves his way, now pondering on the significance of tribal nomenclature and royal genealogy, now turning aside to consider the obscurities of a Zoroastrian or Manichaean fragment, now pausing to assess the value of a recent "dig" or to reflect on the transmission of a stylistic motif along the caravan routes of Inner Asia. Throughout, however, his object is to trace the abiding elements in Iran's elusive cultural identity, and, if there is one particular theme that runs through this important study, it is the sheer resilience of Iranian society over centuries of upheaval and its ability over time to sustain its own, distinctive great tradition.

Tracing the continuities over so great a stretch of time and country raises formidable problems of interpretation, since the sources, more often than not, resist chronological treatment. Frye's great merit is that he has the ability to draw together data from a number of distinct academic disciplines and work them into a meaningful and connected historical narrative. This ability deserves emphasis, since much scholarly writing on ancient Iran seems arcane and hermetic to the nonspecialist.

Frye's writing is most accomplished when his sources provide him with the opportunity for sophisticated interpretation; hence, he is at his best in the Parthian and Sasanid epochs and when he crosses the Oxus into Central Asia. To this reviewer, the first two chapters seem the least satisfactory. The opening geographical survey, superfluous to one who knows the terrain, may appear too perfunctory to one who does not. The second chapter, a review of the ethnography of ancient Iran, relies heavily on data drawn from later periods, a natural temptation in view of the paucity of contemporary information but dangerous, nonetheless. By contrast, chapters 9 and 11, on the Kushans and the Sasanids, are models of how ancient history should be written despite the contraints of the sources.

Frye's extensive publications include two lengthy interpretive studies on early Iranian civilization, *The Heritage of Persia* (1962) and *The Golden Age of Persia* (1975), as well as his more recent contribution to *The Cambridge History of Iran*. Building on his earlier achievements, his latest work provides the English-speaking reader with what is now the best available account of a difficult and important phase in the history of the Middle East.

GAVIN R. G. HAMBLY
University of Texas, Dallas

MEDIEVAL

JOSIAH COX RUSSELL. *The Control of Late Ancient and Medieval Population.* (Memoirs, number 160.) Philadelphia: American Philosophical Society. 1985. Pp. xv, 272. $17.00.

In this book Josiah Cox Russell attempts to discover how the population of Europe between the first and the fifteenth centuries was controlled. To achieve this, he uses, in addition to data obtained from his previous studies, new evidence from Roman gravestone inscriptions and especially from skeletons excavated from Roman and medieval cemeteries. Russell divides the fifteen hundred years under study into five periods and estimates for each one of them the demographic trend, the size of towns, mortality, life expectancy, and other demographic variables. An analysis of the bubonic plague and its demographic effects reveals that, as a result of the outbreaks of the plague in the sixth and fourteenth centuries, the population was reduced by more than 40 percent and that in both periods more than one hundred years were required for it to recover. Although he recognizes that the plague and other infectious diseases had a considerable effect on the population, he refuses to see mortality as the sole factor that controlled population growth.

Russell suggests that during the period under study a self-regulatory demographic regime brought population size in line with resources. He maintains that the mechanisms of population control were the necessity of obtaining adequate means of support before forming a new family, which determined the age of marriage and the proportion of married people according to the availability of resources, as well as female infanticide, which decreased the number of adult women, and the method of feeding babies, which controlled the fertility of married women. He argues, however, that these mechanisms were broken down in the tenth century under the pressure of the feudal barons competing with each other for more men and resources. The rapid population growth that ensued was stopped, according to Russell, not by subsistence crises but by a return in the late thirteenth century, everywhere except in Italy, to the

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old voluntary mechanisms of population control. This happened because medieval people realized that resources were becoming scarcer and because the pressure on them to multiply ceased as the feudal barons were firmly controlled by more efficient kings (p. 272).

Russell is one of the pioneers of modern demographic history, and his previous studies contributed to a better understanding of late ancient and medieval European demography. This book, however, is rather disappointing. Firstly, he uses the main sources for this study, namely, inscriptions and skeletal evidence, in an uncritical way. Although only scientifically excavated skeletons from large cemeteries provide reliable demographic data, he also employs data on skeletons from small cemeteries as well as from unscientifically excavated ones. Consequently, a strong unknown element of bias is introduced into his sample, which distorts his observations. Whereas, for example, the sex ratio estimated from the sample of all the skeletons used by him is as high as 132 (p. 153), the one calculated from the better skeletal data of large cemeteries is only 102 (pp. 155–57). Moreover, although the majority of the demographic observations made in this book are about Western and Mediterranean Europe, the majority (61 percent) of the skeletons studied were excavated in Central and Eastern Europe.

Secondly, Russell's demography is curiously outdated and narrow, since he does not use the considerable amount of data obtained in the last twenty-five years about the economy and the society of late ancient and, especially, medieval Europe. He also ignores the results of the outstanding work done by early modern demographers, which throws much light on the demography of the bubonic plague and other infectious diseases as well as on other demographic characteristics of the preindustrial population.

Thirdly, Russell fails to bring any convincing evidence to substantiate his theory of population control. Changes in the methods of feeding babies, in the number of females killed at birth, in the age of marriage, and in the proportion of married women in the population are assumed rather than proven. His claim that the deep-rooted mechanisms of population control were broken in the late tenth century under the pressure of feudal anarchy is hard to accept given that during the same period there was a rapid population growth in areas like Germany, where a nonfeudal political system prevailed. It is also difficult to accept his assertion that the population growth of the thirteenth century was slowed down by a return to the old preventive checks when all the economic as well as the demographic evidence available shows that it resulted from a marked fall in living standards and a consequent rise in mortality.

Despite all its shortcomings, this book demonstrates the value of archaeological evidence for historical demography, which, if handled in a more cautious and controlled way, will undoubtedly throw further light on the history of the population of late ancient and medieval Europe.

ZVI RAZI
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WERNER RÖSENER. *Bauern im Mittelalter*. Munich: C. H. Beck. 1985. Pp. 335. DM 39.50.

This admirable survey attempts to rectify the neglect of the peasantry by most German scholars. Adopting the conceptual framework suggested by Wilhelm Abel, Werner Rösener focuses on the German peasantry but also includes comparisons with Western Europe. He concentrates on the period between the eleventh and early fourteenth centuries, with shorter chapters on peasant conditions in the early Middle Ages and after the plagues. Early medieval agrarian life was pastoral in considerable measure, with a mobile peasantry owing labor services and less often payments in money and in kind. Criteria of peasant status were juridical. But the increase in population had obliterated many legal distinctions between persons by the eleventh century. German writers thus abandoned the older "free-unfree" classification for that of "estates" of peasant, burgher, and knight. Peasant social status during the central Middle Ages thus resulted from economic differentiation. With higher population densities came a more systematic elaboration of lords' power over the land (*Grundherrschaft*), together with more emphasis on labor-intensive agriculture and payments in money and in kind and less on labor services. Village communities devised bylaws, often at their lords' instigation, to regulate common practice in the now-crowded villages. Although numerous charters of liberties were granted in Germany, they seem to have had less practical impact than in France and the Low Countries.

The development of markets, particularly in the cities, benefited mainly the lords and wealthier peasants, who had a surplus. Most peasants, however, produced primarily for subsistence. Rösener thus considers inheritance customs a more reliable indication of peasant economic potential. In areas of impartible inheritance there was a small upper and middle group and many poor. Partible inheritance is more often found in areas of dense population; it produced a mobile land market and an enormous group of smallholders whose land was insufficient to support a household and who thus had to supplement their incomes by other means.

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Rösener tends to give perfectly reasonable expositions of economic trends, only to fall back on juridical explanations for them. But, aside from his consistent improper use of "feudal" instead of "seigneurial," he eschews controversy and presents recent and generally held scholarly opinions. In addition to the more analytical section of his book, he provides a wealth of information on farm technology, the natural environment, work routines, payments and obligations, diet and clothing, the significance of the nuclear and extended peasant family, village and farmstead layouts, and the architecture of peasant homes. Noting that archaeological evidence tells us more about the prosperous, whereas the written evidence is often better for the poor, he attempts a judicious balance, drawing on both kinds of evidence. I would like to see an English edition of this book, perhaps with some expansion of the sections dealing with Germany's neighbors, for use in the classroom.

DAVID NICHOLAS
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C. STEPHEN JAEGER. *The Origins of Courtliness: Civilizing Trends and the Formation of Courtly Ideals, 939–1210.* (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1985. Pp. xiv, 326.

C. Stephen Jaeger has written a book marked with the touch of mastery: it is notably well written, presents a deeply original thesis, and is likely to prove seminal. He is searching for the origin of that code of behavior, evident in the portrayal of the ideal knight, that made courtesy an essential partner with courage and that he identifies as the most important restraining and civilizing influence on the violent tenor of medieval aristocratic life. He convincingly locates that origin in the German cathedral schools and the royal chapel of the Ottonian and Salian periods, in which that vital political elite of the time, the future imperial episcopate, was trained. Using the vitae of celebrated bishops as a source, he identifies a series of qualities clearly perceived as keys to advancement in the career of an aspiring *clericus*. The collocation of these qualities, intrinsic to the making of an ecclesiastical statesman, owed much to the influence of Ciceronian ideas, especially to the *De Officiis*. They included good birth, handsome appearance, affability, and popularity, in combination with erudition, gentleness of spirit, and a reticence in the display of talent that was often explicitly calculated. In the vitae these became the *topoi* of descriptions of how, as aspiring young men, their subjects first made their mark.

From this point Jaeger looks forward to the romances of the later twelfth century (in particular to Gottfried's *Tristan*) and to contemporary *Stifterchroniken* (like those of John of Marmoutier and Lambert of Ardres) to reveal these qualities reappearing, alongside the traditional martial virtues, as *topoi* in descriptions of young noblemen and knights. He shows not only that the chroniclers used here the same Latin vocabulary as that of the older *vitae episcoporum* but also that there is a close, sometimes almost one-to-one relationship between their recurrent Latin words and phrases and the topical usage of the vernacular romance writers (*disciplina/zuht, elegantia morum/schoene site*, and so on). His case—that the courtly qualities that came to be viewed as essential to civilized knighthood were originally conceived as identifying the aspirant cleric and that it was the courtly clerics who began the business of teaching good manners to unruly chivalry—seems to me conclusive. This role of the *clerici* has not before been given due emphasis by historians of chivalry and, as Jaeger argues, reveals a new dimension of Latin and classical (especially Ciceronian) influence on the vernacular portrayal of the ideal knight.

Jaeger explores with insight and scholarship the problems of the dissemination and reception of courtly ideals; the thorniest of all is why it is that, although the courtly qualities first appear in German episcopal vitae, it is in French romances and chronicles that we first find them associated with secular aristocrats. The author's numerous perceptive suggestions on this and other issues open broad areas for further inquiry. This is a book that constantly prompts significant questions. It seems to me the most original contribution to the ideological history of medieval aristocratic mores that has appeared for some time.

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University of Oxford

CHRISTINE FELL *et al. Women in Anglo-Saxon England and the Impact of 1066.* Bloomington: Indiana University Press. 1984. Pp. 208. $24.50.

Scholars of medieval history will be happy with Christine Fell's learned contribution to the much-discussed question of the role of women in early England. This modest and unassuming book is filled with useful information wisely organized around accessible and familiar topics—women in daily life, sex and marriage, family life, and women's roles in court and in religious houses. Viking women in England merit a chapter unto themselves, and two concluding chapters by Cecily Clark and Elizabeth Williams discuss documentary evidence and literary sources after 1066.

The authors note at the end of their introduction that it is "salutary to remember that scholarship does

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not require us to read only, always and inevitably a history of oppression and exploitation of the female sex. The real evidence from Anglo-Saxon England presents a more attractive and indeed assertive picture" (p. 21). This forthright statement sums up their refreshing view of the subject. They are not, of course, the first to suggest that women held positions of power and influence in early English culture, and they cite the views of many earlier scholars who believed that in Anglo-Saxon times women stood on equal footing with men in many matters.

The range of evidence is impressive and the presentation forceful. Archaeological detail is prominent, but other sources are not slighted. A few well-chosen words about additions to King Alfred's translation of the *Consolation of Philosophy*, for example, suggest the king's sympathy for the plight of wives whose husbands were taken from them (p. 73). The chapter on "factual evidence" after 1066 (by Clark) is exceptionally trenchant and contains an excellent summary of the evidence from canon law.

The book is richly illustrated, although the illustrations are not helpfully documented. Manuscripts are identified only among the photographic sources listed on pages 202–03, information that should have accompanied the illustrations; manuscripts are also not always dated. The textual documentation may be frustrating to some students and nonspecialists. Instead of notes, a few general references follow the end of each chapter. A comprehensive bibliography for the first seven chapters is just four pages long, although another four pages contain the bibliography for the last two chapters.

The authors observe that they had to omit much material and that each chapter could have been turned into a separate volume (p. 21). We can be grateful that they have not done so, for the chief strength of this book is precisely that it covers so much evidence authoritatively and compactly.

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R. H. HILTON and T. H. ASTON, editors. *The English Rising of 1381*. (Past and Present Publications.) New York: Cambridge University Press. 1984. Pp. vi, 220. $37.50.

A radical change in the historiography of the English Peasants' Revolt of 1381 may be dated from the publication in 1970 of R. B. Dobson's volume by that title. In the introductory survey to that study, Dobson concluded that one hundred years of historiography had produced no satisfactory explanation of the 1381 revolt, and he suggested that local and regional studies may produce more satisfactory results. The main contributions of this slim volume, comprised of six papers from the Past and Present Conference of 1981 together with two additions (by Dobson and Alan Harding), does indeed lie in the direction envisaged by Dobson in 1970.

The most substantive detailed study in this volume is that by Christopher Dyer, a report on the state of research on a project to compile biographical studies of individual rebels from manorial and central government sources. The immense size of the project can be seen from printed sources showing that 105 villages were involved in Essex, 35 in Hertfordshire, 118 in Kent, and 72 in Suffolk. Although Rosamund Faith's study focuses on the better-known peasant movement of 1377, the fact that this involved at least forty villages in Wiltshire, Hampshire, Surrey, Essex, and Devon underlines the validity of her selection. When it is added that A. F. Butcher's study includes Canterbury, Dobson's York, Beverley, and Scarborough, and Harding's the "home counties," the geographical coverage of this volume is impressive indeed.

Perhaps the most significant advance in methodology among these studies may be found in the chapter by Butcher. After noting in his introductory remarks that there has been "little detailed study of fourteenth-century English urban society at all," Butcher proceeds to examine Canterbury and to conclude that "the so-called alliance between townsmen and villagers in 1381 was not, in Canterbury's case, a temporary phenomenon but a natural expression of a regional social structure in which town and countryside were inextricably entwined" (p. 110). The most suggestive new lines of analysis for interpretation of various revolts, however, are in the chapters by Dobson and Harding. Dobson demonstrates that the new mercantile environment shifted the source of wealth from the local scene and widened the gap between the poorer local and regional population and the overseas merchants. Harding's chapter, "The Revolt against the Justices," complements that of Dobson by demonstrating a legal evolution for one hundred years prior to 1381 that gradually brought legal jurisdiction into the hands of the magnates. Although Harding's study pulls together a long tradition of legal scholarship on such points as the justices of the peace, the carefully coordinated arguments of his chapter make this an essential point of departure for future interpretations of the 1381 revolt. The conclusions drawn by J. A. Tuck about the differing reactions to the revolt by nobles and commoners explains, no doubt, why the evolution of various factors developing before 1381 must be traced through subsequent centuries for their fuller comprehension.

The two comparative studies from the Continent—"The Jacquerie" by Raymond Cazelles and "Florentine Insurrections, 1342–1385, in Comparative Perspective" by Samuel Cohn, Jr.—although

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interesting in themselves, add little to the volume except perhaps by providing further evidence for the inadequacy of traditional interpretations of revolts. But this is a point already well demonstrated by most of the studies about England. As one of these studies after another draws on a century of various tensions and revolts, it becomes clear that the historian is now dealing with a century of fundamental change in all institutions of society. Efforts by several of these contributers to "save" the traditional class war explanation of economic and Marxist historians deflect attention from the obvious need to identify more clearly various elements of that society prior to these changes. Dyer's analysis of peasant reactions in the early fourteenth century, for example, might have more meaning had he acknowledged that there was a peasant culture with sensitivity to contractual rights rather than freedom prior to what he calls the "second serfdom" after the Black Death. On the same theme, Faith's juxtaposition of the "two economies" of lord and peasant is too simplistic in view of the recent research by Bruce Campbell and John Langdon on the sophistication of peasant technology. More generally, the surprise of several scholars about the rural-urban complexity of peasant activities could have been mitigated had there been more awareness of the long-established insights by anthropologists that peasant culture is a "part" of a larger whole.

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DEREK KEENE. *Survey of Medieval Winchester.* In two volumes. Contribution by ALEXANDER R. RUMBLE. (Winchester Studies, number 2.) New York: Clarendon Press of Oxford University Press. 1985. Pp. xxxviii, 448; 452–1490. $198.00.

This massive and magnificently produced work is the second major installment of Winchester Studies, the series prepared by the Research Unit of the Winchester Excavations Committee under the general editorship of Martin Biddle. This set covers the later Middle Ages, from about 1300 to the mid-sixteenth century; the companion volume, *Winchester in the Early Middle Ages*, appeared in 1976. Together they provide, through the integration of documentary and archaeological evidence, a topographical and historical record of a medieval English community unrivaled in richness of detail.

The scope of Derek Keene's undertaking is astonishing. The basic reference materials are laid out with meticulous and loving care in the thousand oversize pages that comprise volume 2. First comes a comprehensive gazetteer (pp. 449–1132) of over eleven hundred sites, detailing the descent of every individual property and parish church, grouped by streets and districts. Maps of each street in the years ca. 1300, 1417, and ca. 1550, along with the cumulative citywide maps included with volume 1, provide graphic illustration of settlement continuities and changes. Most of the rest of this volume is then devoted to a biographical register (pp. 1133–1403), providing information on nearly nine thousand inhabitants and property holders in this two-hundred-fifty-year period. Keene has achieved nothing less than the definitive reconstruction, both physical and human, of later medieval Winchester. Social and urban historians have in this volume a repository of data few medieval scholars would have dared attempt, much less thought possible to assemble.

Volume 1, abundantly supplied with further important documentation, is essentially an introduction to, and detailed commentary on, this material. It is informed and enlivened by one overriding theme: Winchester's decline. Although the bishopric remained wealthy, it came increasingly to dominate civic life because of the decay or diminution of other traditions—St. Giles's Fair, the role of the city in the wool trade and clothing industry, and in particular its once-lustrous status as a provincial site of royal governance. Keene is confessedly cautious in his estimates of population and demographic trends, but it seems clear that the Black Death may have halved the population to levels that remained permanently under ten thousand. Economic enterprise became increasingly localized, and, even though the number of functioning churches declined, a steadily increasing proportion of civic wealth and energy was directed to ecclesiastical purposes. Secular material impoverishment, however, must not be unduly stressed. Late medieval Winchester did retain an honorable, if diminished, place in national life, and the relative absence of extreme discrepancies between the rich and the poor has enabled Keene to demonstrate convincingly, without sentimentalism, a continued vitality of civic patriotism and sense of community.

Beyond the achievement of Keene and his collaborators, a further congratulatory note must go to Oxford University Press. The glories of Saxon and early post-Conquest Winchester may long since have dimmed, but the glories of the printer's and the publisher's art, on the evidence of these volumes, have rarely been brighter.

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PENNY SCHINE GOLD. *The Lady and the Virgin: Image, Attitude, and Experience in Twelfth-Century France.* (Women in Culture and Society.) Chicago: University of Chicago Press. 1985. Pp. xxiv, 182. $20.00.

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Penny Schine Gold offers a striking description of the lady and the virgin. She analyzes the two conflicting images of the secular lady in French literature of the twelfth and early thirteenth centuries and demonstrates how these images were complemented by the portrayal of the Virgin Mary in Romanesque and Gothic iconography. Then she notes how the female monastic experience corresponds to these appearances. But, when she tries to investigate women's control of the family's property, her argument breaks down.

Gold regards the *Song of Roland* as the least typical of its genre because it does not offer examples of extensive female action; she therefore examines women's role in epic poetry through the oldest poem, the *Song of William*, and the most popular song of revolt, *Raoul de Cambrai*. The conclusion she draws is that women act only on the sideline but are motivated by the same goal as that of the hero—upholding the family and its feudal property. Women nourish and advise men but are unable to fight their battles. At the same time that *Raoul de Cambrai* was written, Chrétien de Troyes produced his *Yvain*. This romance, together with several others, shows a different sort of relationship between men and women. At the center of the tale stands the man; he has to prove his worthiness in the eyes of other men and has to balance this courage by winning the love of the lady. This time not his family but his own worth is at stake.

These two literary expressions of women's subordination to men, one depicting women as helpmates and the other as objects of devotion, had their counterparts in religious art. Mary seated with Jesus, the traditional twelfth-century Romanesque image, was superseded by the figure of Mary standing, holding in her arms the Child. The artists also transposed the Virgin and Son adored by the Magi into the Virgin lifted up into heaven and seated next to her Son, not as mother but as the *sponsa* from the *Song of Songs*. Thus, for a brief moment Mary became equal to her Son in the tympanium. But later Gothic sculpture does not accomplish this transformation; the Madonna is still in heaven, but she is the humble Virgin who received the crown in complete meekness from Christ, who is her bridegroom.

These complex and ambivalent images find their parallel in female monastic experience. Early in the twelfth century, women were generally accepted in the newly founded male orders, but, toward the end of the century, they were excluded whenever it was possible. Fontevrault was a notable exception. It was founded by Robert of Arbrissols with the notion that the two sexes should live together. Robert carefully delineated the role of the nuns and canons, putting the double monastery under the rule of an abbess. Thus, the reaction that occurred later in the century against female members of the orders could not adversely affect Fontevrault. The orders' reluctance and later their refusal to accept women was in the same spirit that promulgated women's subordinate position in literature and art. Authors of the epic poems and romances as well as the artists who worked on the churches were men whose relationship to women was riddled with ambiguities. On the one hand, they belittled and blackened the character of women, representing them as the source of sexual danger and of added responsibility for men. On the other hand, they could still be friends with individual females.

As sophisticated as her conclusions are in the preceding parts, the last section of Gold's work leaves something to be desired. As a historian, she ought to be at home with quantitative analyses. Indeed, she notes at first that quantitative investigations may be important, but then she does her best to denigrate their usefulness. For example, she states that women in the Middle Ages could not expect to participate in transactions that concerned their own property—their dowry, dower, or inheritance—unless they were widows. And she cites some cases in which this indeed occurred. But these were exceptions found in wills, for otherwise a man would not differentiate between the woman who was his wife and coactor or merely his future heiress. The latter approved with her signature the alienation of this property. The transactions reflect the rough pattern of landownership, and for some periods this is the only known pattern. Should historians be prohibited from analyzing this evidence, especially if they do so on the basis of a chi-square test, taking the figures only at 95 percent confidence level? Moreover, the end of the book has a disappointingly weak conclusion that women's control of property "peripherically" as members of a family was reflected in their secondary role in the *chanson de geste*. This judgment could also have been reached by taking more seriously the available data.

I would recommend the book but with the expectation that any reader interested in quantitative history would look up the data in the cited references and would also read the various authors' comments. By encouraging free access, especially to a work that has such great interdisciplinary merits as this one, we make historians more aware of their future task. The book's footnotes provide an excellent directory to recent works.

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LEAH LYDIA OTIS. *Prostitution in Medieval Society: The History of an Urban Institution in Languedoc*. (Women in Culture and Society.) Chicago: University of Chicago Press. 1985. Pp. xvii, 240. $22.50.

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Prostitution is a subject that has been neglected by the professional historian. Even when books on this topic have been reviewed in the *AHR*, as an earlier study of mine was (*AHR* 70 [1964–65]: 834–35), the reviewer made fun of the topic and complained that the book did not titilate him. Times, however, do change, and one of the results of the new feminist scholarship has been a concern with neglected areas of women's occupations and roles. This study by Leah Lydia Otis is an effective illustration of the changes. It grew out of her Ph.D. dissertation on a topic that earlier would have been "officially discouraged" by an adviser; it is published by a distinguished university press; and it is part of a series on women in culture and society.

Our knowledge of prostitution is haziest for the medieval period, and this study is an excellent example of what can be found from a diligent search of the records. It is based on a careful analysis of texts: Otis also examines reasons for accepting or rejecting their authenticity (in the sex-affiliated areas there are many forgeries) and places them in historical context. In spite of the title, the study is a regional one and not of medieval society in general; nonetheless, the result is a revision of some of the traditional versions of medieval prostitution. The author found evidence for the existence of prostitution in the early Middle Ages, but only for the twelfth and thirteenth centuries are the sources plentiful enough to describe it in any detail. By the fourteenth century prostitution was institutionalized, and in the sixteenth century these institutions were being dismantled. The last section, on the sixteenth century, is the weakest part of the book, for the author's attempt to explain changing attitudes indicates a lack of understanding of the concepts of contagion and the effects of sexually transmitted diseases. Otis is correct, however, in emphasizing a multiplicity of reasons, including changing moral standards, associated with the rise of Protestantism and the Catholic reform movement.

The narrative portion of the text is about one hundred pages. The rest of the book is devoted to notes and to documents in both Latin and French, lists of prostitutes and brothel operators, graphs of lease prices for brothels, and maps locating privately and publicly owned brothels in Languedoc. The bibliography is good, but it has a number of lacunae. Overall, the work is a significant contribution to our understanding not only of prostitution but also of urban life and woman's role in the late medieval period.

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STEPHEN C. FERRUOLO. *The Origins of the University: The Schools of Paris and Their Critics, 1100–1215*. Stanford: Stanford University Press. 1985. Pp. viii, 380. $45.00.

This study proposes a bold reinterpretation of the origins of the University of Paris. Opposing current thinking represented by Alexander Murray's *Reason and Society in the Middle Ages* (1978), which sees the medieval universities as resulting from increased specialization, careerism, and competition, Stephen C. Ferruolo draws inspiration from Hastings Rashdall's classic, *The Universities of Europe in the Middle Ages* (1895), and maintains that university men were motivated by a common ideal to join together harmoniously in a single corporation to advance teaching and learning. After an initial chapter sketching the schools in the early twelfth century, Ferruolo supports his theme by examining carefully the critics of the Paris schools. Bernard of Clairvaux and his monastic followers squarely confronted the schools with the competing ideals of the cloister. Literary satirists like Walter of Châtillon, John of Hauteville, and Nigel Wireker vented the frustrations of their careers by attacking the ambition and cupidity of successful scholars, particularly those in medicine and law. The three renowned humanists, John of Salisbury, Peter of Blois, and Gerald of Wales, likewise thwarted in their personal careers, voiced similar criticisms. Their remedy was to moderate specialization with a broad training in the liberal arts of grammar, logic, and rhetoric. Most critics were agreed on the moral superiority of studying theology over logic, law, and medicine. All of this terrain is indeed familiar to students of medieval learning. As Ferruolo himself acknowledges, his work relies heavily on studies, critical editions, and even excellent English translations that have appeared in the past generation but with which he is frequently dissatisfied. His treatment of this material is lucid, rising on occasion to brilliance, as when he interweaves the writings of the humanists with their lives in the best tradition of Richard W. Southern.

The original contribution of the work, however, lies in two subsequent chapters devoted to preachers who admonished congregations of masters and students at Paris in the second half of the twelfth century. Benefiting from Jean Longère's compilation, Ferruolo has investigated the sermons of eighteen theological masters, some of the eminence of Peter Lombard, Peter of Poitiers, and Stephen Langton. From this corpus of unexplored material, much of it still in manuscript, he has searched for the impulses lying behind the formation of the University of Paris. His findings uncover attitudes not greatly different from those of the earlier critics. Although the theologians inveighed long and loudly

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against curiosity, vanity, and cupidity, they proposed little positive advice apart from exhorting their learned audiences to be good Christians and from asserting the unity of all knowledge subjected to the superiority of their own discipline. Except for the well-known letter of Stephen of Tournai addressed to the pope (not a sermon, incidentally), their preaching was reticent on specific measures that might constitute a reform program.

Reform, however, did surface at Paris in the opening decades of the thirteenth century. The masters of all disciplines took the first step to form a single corporation by 1209, and the papal legate Robert of Courson drew up statutes minutely regulating curriculum and behavior by 1215. Ferruolo draws close connections between the preachers' sermons and these reforms, but it is difficult to follow his argument. Nowhere do the preachers openly propose to join together in a single body. On the contrary, like the earlier critics they continue to voice competition with the lucrative professions of law and medicine. Their platitudes touch few of the concerns detailed in Courson's statutes. As clues to the origins of the university, therefore, the theologians' sermons disappoint. It would be better to return to the lectures and disputations of contemporary masters like Peter the Chanter, Stephen Langton, and Robert of Courson, which, whatever their inadequacies, were more acutely sensitive to the contemporary scene. Compare, for example, Courson's nuanced discussion of collecting fees from students with Odo of Soisson's pious hope that scholars will not enrich themselves from teaching. Although Ferruolo's book may be read with profit for its individual parts, his overarching argument fails to convince. Tending to emphasize single elements at the expense of others, his procedure obscures rather than clarifies complex situations. The theologians may well have thought that they were preaching harmonious ideals when they asserted the preeminence of theology, but their colleagues, the logicians, doctors, and lawyers, cannot be blamed for seeing this as specialization, careerism, and competition.

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SHERRY L. REAMES. *The* Legenda aurea: *A Reexamination of Its Paradoxical History*. Madison: University of Wisconsin Press. 1985. Pp. viii, 321. $40.00.

The *Legenda aurea* was written between 1255 and 1266 by the Dominican friar Jacobus de Voragine. It consists mainly of vignettes of saints' lives and short essays about Christian festivals arranged in accordance with the church calendar. Its purpose was to foster piety among the faithful. From the beginning, the *Legenda aurea* was a "best seller" that enjoyed a wide popular audience until the sixteenth century, when, because of Renaissance scholarly criticism of hagiographic evidence coupled with Protestant aversion to Catholic devotional literature of this genre, it disappeared from the printing lists almost overnight.

Following the Latin edition, the *Legenda aurea* was translated into every major European language, abridged for a shorter version, and used as a model by other authors who wished to copy the popular genre. The extant manuscripts number around eight hundred, according to Sherry L. Reames. The advent of the printing press added to the popularity of this masterpiece, which became a staple of printing houses everywhere. Between 1470 and 1500 at least 156 editions were printed. In the course of the sixteenth century, the *Legenda aurea* lost its popularity, however, and the last edition was printed in Italy in 1613; in the next 230 years only one new edition appeared. In the past one and a half centuries the text has enjoyed a comeback and has been printed in most major European languages. Today it is considered a classic in devotional reading.

Reames records the fascinating tale of the vicissitudes of the book's fortunes. She searched diligently for explanations of this phenomenon and sought to provide an insight into the book's popularity (or lack thereof) through the ages. The story is complex, and she handles it through three interconnected parts. Beginning with an analysis of the Renaissance criticism of the *Legenda aurea* in order to explain what the book really was and was not, how medieval writers such as Jacobus handled hagiographic evidence, and how Renaissance scholars appraised such evidence, she moves to case studies of selected saints' lives. Her primary example is the life of Saint Benedict as portrayed by Gregory the Great in the *Dialogues* and by Jacobus in the *Legenda aurea*. Reames's findings are that Jacobus departed from and abridged Gregory's version in order to provide preachers with ammunition against current enemies of the church. She then continues a comparison of Jacobus with other writers by focusing on three more cases—Ambrose, Augustine, and Dominic—to show Jacobus's purpose and selectivity.

This is an interesting study of hagiography and of the way in which hagiographic texts became popular. It also presents interesting comparisons between medieval and Renaissance notions about scholarly evidence. Reames cautions teachers that the *Legenda aurea* is not representative of its genre and that frequently it is assigned to students to read when much better works are available. The omissions and biases of Jacobus de Voragine, she finds, have valuable lessons to teach more advanced students.

The end notes are copious, numbering almost as many pages as the text, and they contain valuable

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information that supplements the text. The book is interesting and is recommended reading for anyone interested in saints' lives, the history of books, and general medieval and Renaissance comparative thought.

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JOHN HINE MUNDY. *The Repression of Catharism at Toulouse: The Royal Diploma of 1279*. (Studies and Texts, number 74.) Toronto: Pontifical Institute of Mediaeval Studies. 1985. Pp. xiv, 336. $28.00.

In 1271, when Toulouse came under direct royal control, attempts were made to enforce Saint Louis's legislation about the confiscation of the property of condemned heretics. This policy caused great resentment, and as a conciliatory measure Philip III in 1279 granted an amnesty to the heirs of 278 citizens convicted of heresy before 1259. John Hine Mundy publishes this diploma in full for the first time, and about a third of his book is concerned with evaluating it. Where possible, he gives brief biographies of the people named in it. He shows that they had all been convicted of Catharism and argues that the diploma was intended to be an exhaustive list of all citizens whose sentences had entailed confiscation of property. It is not, of course, a list of all the convicted Cathars of Toulouse, only of those who owned property. Mundy shows that the majority (195) of the convictions occurred before 1238 whereas only 26 date from after 1248; he thus concludes that support for Catharism among the rich families of Toulouse was effectively broken within a decade of the Peace of Paris, before the Inquisition had become well established.

This is debatable: the penalty of confiscation did not attach to all convictions for heresy but was restricted to sentences involving the death penalty, to certain categories of imprisonment, to those found guilty of relapsing into heresy, and to those condemned for refusing to appear before a church court. Inquisition evidence shows that sentences involving confiscation were less common after the inquisitors had devised a regular procedure in the 1240s. Philip III's diploma merely confirms this; it does not necessarily prove that there were fewer Cathar sympathizers among the propertied classes of Toulouse after 1248, only that they were less severely punished. The rest of the book consists of studies of twenty-two of the families named in the diploma. Mundy has drawn on a lifetime's knowledge of the southern French archives to present a picture of family relationships, property holdings, political power, and religious allegiances within these groups. It is clear from these studies that Catharism had penetrated many of the greatest Toulousan families before 1229, but it is equally clear that few of them can be labeled "Catholic" or "Cathar," since both religions coexisted in most kinship groups. Thus, while the main branch of the Castronovo family favored Catharism, some of their cousins supported Bishop Fulk's *Confrérie Blanche*. Intermarriage between families of Catholic and Cathar sympathies was also quite common.

All this helps explain how Catharism became so widespread in Toulouse during a single generation in the late twelfth century. These family histories are valuable for a much wider range of studies than that of religious history, and Mundy's book will be an essential reference work for anybody studying southern French history in the twelfth and thirteenth centuries. A minor mistake has been made in dating the diploma (p. 75): the feast of the Decollation of Saint John the Baptist fell on August 29, not August 25.

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HEATH DILLARD. *Daughters of the Reconquest: Women in Castilian Town Society, 1100–1300*. (Cambridge Iberian and Latin American Studies.) New York: Cambridge University Press. 1984. Pp. xii, 272. $49.50.

Over several centuries the kings of Castile and León expanded their realms, moving their frontiers from the Duero River in the north to the Guadalquivir in the south. This process of reconquest was accompanied by repopulation or colonization. In a broad area of Old Castile, León, and Castilian and Leonese Extremadura, the characteristic settlement was the municipality, a walled town with jurisdiction over an extensive rural area. Most of these towns received royal charters, or *fueros*, that regulated the mode of government, tributes, military obligations, contracts, and everyday affairs. These *fueros* serve as the foundation of Heath Dillard's study and yield a surprising amount of information about the lives of women.

Dillard's fundamental point is that the presence of women in the towns was a guarantee of permanence and stability. Consequently, every effort was made to attract them as wives for the warriors who conquered the area and were establishing the town. The lives of women from betrothal through marriage and childbirth to widowhood are clearly reflected in the charters, as is the contrast between secular law and the developing canon law of marriage. To parents, the ideal was that their daughters married men of substance and social position. Through marriage the daughters were assured of protection and also enjoyed certain rights and privileges. Wives were engaged in a joint enterprise with

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their husbands, sometimes working at the same trade or cultivating their gardens and fields outside the town walls. Ordinarily women were involved neither in military matters nor in civil government, although Dillard suggests that wealthy women probably had some influence in town affairs. Most women went about a daily routine of housekeeping, shopping for necessities that the family did not produce itself, making goods to sell, such as bread, and maintaining contacts with friends and relations.

The hazards of life in a frontier town were very real, especially when the men were away on a military expedition. Aside from the possibility of capture by the Moors and eventual enslavement was the threat of sexual abuse by Christian neighbors. Abduction of wives evidently was common, although it is likely that the girl, because of parental opposition to her marriage, was often a willing accomplice of her abductor. Curiously, while some towns were willing to admit an abductor and his bride from another town, they dealt harshly with anyone who abducted one of their own girls. Unmarried women (*barraganas*) who maintained a stable relationship with one man had certain legal standing but were often looked down on (especially the priest's *barragana*) by the upstanding married women of the community. Prostitutes, procuresses, and sorcerers, however, were social and legal outcasts.

This book is well written and makes excellent use of a great number of *fueros*. A study of the Portuguese and Aragonese *fueros* would likely offer a similar picture. As lawbooks, these charters tell us much about the medieval development of male attitudes toward women—attitudes that still persist in many quarters. What is missing from the *fueros*, and consequently from Dillard's book, is the personal history of women that could be gleaned from the records of purchases and sales, bequests to religious houses, lawsuits, and the like. The compilation of that sort of evidence is difficult, but it is a task that should be undertaken. Even so, this book is a major contribution to the social history of medieval Castile-León, because it opens an entirely new vista on the process of colonization of the towns.

There are extensive notes, a bibliographical index to the *fueros*, an index, and illustrations from the thirteenth-century *Cantigas de Santa Maria*.

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WILLIAM H. TEBRAKE. *Medieval Frontier: Culture and Ecology in Rijnland*. College Station: Texas A&M University. 1985. Pp. xiv, 293. $23.50.

William H. TeBrake takes as his subject a coastal section of the Rhine delta flanking the now-blocked mouth of the Oude Rijn—a region of sand dunes and marine clays on the seaward side, alluvial clays along the river course, and a considerable expanse of what used to be raised peat bog inland. TeBrake describes the reclamation for arable of this peat bog in the twelfth and thirteenth centuries and goes on to show how, by the fifteenth century, much of the area had already subsided below sea level and the underlying clays had been exposed. Productivity declined but costs escalated as dams, canals, ditches, banks, sluices, locks, and windmills had to be progressively introduced and maintained.

More than two-thirds of the book is devoted to a survey of settlement in the Rijnland from the fourth millennium B.C. to A.D. 1000, accompanied by an exceptionally clear account of hydrographic changes in the same period. The Rijnland, however, was abandoned in the four centuries before the millennium, so there was no cultural continuity between the premedieval inhabitants and the tenth-century colonizers, let alone those who followed in the twelfth. In retrospect, the author could have summarized this conclusion more succinctly and given greater weight to the map-making centuries after A.D. 1500, which would have furnished greater insights into the period of colonization. No doubt, lack of time and funds precluded this.

In general, historians pay too little attention to physical geography and agriculture, but, in a commendable effort to rectify this, TeBrake has perhaps gone too far in neglecting the documentary base. Neither description of the sources nor apology for the omission of the later sources and the comparative material is given. For example, the author does not mention D. Nicholas's work on Weert in Flanders, nor those of H. C. Darby, H. E. Hallam, N. Nielson, and M. Williams on the reclamation of the peat fens in East Anglia and Somerset. The result is a certain vagueness at the very points where precision has come to be expected. For example, what evidence is there that the *Landgemeinde* system was employed in the Rijnland? Much hinges on this single point. Firstly, the system implies considerable direction from above; who, precisely, were the seignorial lords who supplied it? Secondly, the system entails nondivisible holdings giving rise to primogeniture, large households, a sluggish land market, and no widow remarriage, none of which are characteristics of the surrounding marshlands at this date. West Friesland in particular was characterized by cognatic inheritance customs, nuclear households, an exceedingly active market in small parcels of land (usually leaseholds), and widow remarriage. It is difficult to elucidate the demography of the area without clarification on these points. Again, thirty-five acres is a generous holding for a peasant family in the thirteenth century. Was the family an extended one, or was the peat too acid to provide adequate crops, or was the holding in-

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tended to be subdivided? In view of the economic pressure to subdivide—owing to population pressure before 1348 and the need to increase the number of ditch- and dike-labor rents as the hydraulic system was expanded in the fourteenth century—it would have taken strong lordship and an even stronger administrative system backed by legal sanctions to prevent subdivision. The documentary evidence thus generated should have survived. Finally, we are told little about St. Martins. Did the monks, renowned elsewhere for their initiative and skill in land drainage, really make no contribution in the Rijnland?

In short, the authors of regional social histories have a demanding readership. Rural sociologists, agricultural, demographic and environmental historians, the experts on place names, and the daters of hedges all depend on the hard data provided by regional studies to carry forward their own surveys. To satisfy them all would not be possible. Nonetheless, the Texas A&M University Press is to be congratulated on selecting this book publication and on the high standard of design and for editorial thoroughness: proper footnotes, an index, a good bibliography, and an excellent and conveniently placed series of maps are a laudable achievement in these days of precipitate decline in publishing standards.

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MARCO POZZA. *I Badoer: Una famiglia veneziana dal X al XIII secolo.* (Collana della Facoltà di Lettere e Filosofia dell'Università di Venezia, Materiali e Ricerche, number 3.) Padua: Francisci. 1982. Pp. 141. L. 12,000.

ROBERTO ZAGO. *I Nicolotti: Storia di una comunità di pescatori a Venezia nell'età moderna.* (Collana della Facoltà di Lettere e Filosofia dell'Università di Venezia, Materiali e Ricerche, number 10.) Padua: Francisci. 1982. Pp. 273. L. 16,000.

Both these volumes result from studies undertaken at the University of Venice. *I Nicolotti* represents Roberto Zago's *tesi di laurea* (1980) directed by Gaetano Cozzi. The Nicolotti were the residents and members of the Comunità di San Nicolò and of Angelo Raffaele dei Mendicoli. The *comunità* was a semiautonomous corporation composed mainly of fishermen who possessed recognized privileges in the catching and marketing of fish. These privileges were actively defended, with the support of the Serenissima for the most part, against the encroachments of fishing interests elsewhere in the Dogado from the dim beginnings of the community until the nineteenth century.

The corporate structure of the Nicolotti took the form of a *gastaldo* elected for life, who worked with a board of twelve *presidenti* in addition to the voting membership assembled in the *Capitolo Grande*. The parallels between the structure of the fishermen's corporation and that of the Venetian governmental corporation are immediately apparent to anyone with even a passing acquaintance with Venetian history. So, too, is the fact that the *gastaldi* and *presidenti* tended to come from the oldest and most prestigious families of the community. The *gastaldo* oversaw the market in fish; the *presidenti* formulated laws for the corporation and acted as a court of first instance.

In the course of Venice's history, the government became increasingly interested in regulating the victualing trades. By the sixteenth and seventeenth centuries, therefore, Zago finds that the *gastaldo* was a vehicle of the Serenissima's policies, although in the crisis atmosphere of 1597 the *presidenti* acquired greater supervision of the *gastaldo*. Governmental concern to limit fishing for what we would term reasons of conservation ran into resistance from the Nicolotti, who succeeded in gaining exemption from such rules. The cohesion of the community also withstood challenges from other interests within the guilds of retail merchants and of food merchants. The growing rejection of guild corporatism in the eighteenth century promoted fiercer attacks on the fishermen's position, but, because they operated only with traditional concepts and thus lived in the past, as Zago puts it, the Nicolotti did not concede the obsolescence of their privileges and mobilized to resist as best they could.

Zago's book is built on meticulous archival scholarship but suffers from conceptual anemia. It is an institutional history. The wider world of Venice and its history comes into play, but only as something intruding on the Nicolotti from outside. Yet what was the effect of the Nicolotti on Venice's development? What were the relations of the Nicolotti with the Serenissima, as opposed to those when their privileges were under attack? How active were the leaders of the Nicolotti in Venetian governmental bodies? Zago has not broken down the isolation of this community, which pursued its peculiar occupation in a corner of the city.

Marco Pozza's book suffers from similar problems. It, too, is based on deft archival research. But he aims at nothing more than a prosopographical reconstruction of one branch of the eminent and ancient clan of the Badoer—a branch that migrated from San Giacomo di Luprio to Santa Giustina at the end of the thirteenth century. The consistent presence of members of this clan among the elite of Venice is indeed impressive. With first notice of an Ursus Badoarius in 900, the Badoer already appeared at important public events. By 1100, several were closely associated with the doges and held

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office as *iudices*. This power and prominence continued through Manasse Badoer (in the late twelfth century) and his descendants, especially Marco Badoer, who forged marriage ties with the Ziani, a clan that died out leaving their fortune to the Badoer. Marco, who invested in land and finance rather than in trade, failed to realize the ambition to be doge that Pozza attributes to him. Later, Badoer fell victim for his part in the Querini-Tiepolo conspiracy of 1310. Not surprisingly, thereafter the family's fortunes declined, and, although some Badoer remained prominent to a degree, Pozza does not follow them past their glory days. Rather, he remains content with the mundane conclusion that the Badoer were fairly typical of Venetian patrician families, building their fortunes on a combination of land, commerce, political office, and marriage alliances; he does concede, however, that Marco's quasi-feudal landlordship around Padua was a departure from usual Venetian fashion.

Pozza and, to a lesser extent, Zago do not insert their work into an historiographical context. It is disappointing that Pozza does not relate his work to that of historians like James Davis, and Zago notably fails to relate his work in a consistent fashion to that of his mentor, Cozzi, much less to that of Brian Pullan, Frederic Lane, and others. Their maiden efforts thus suffer from an overly fetishistic approach to the documents. Entranced by the minutiae of their discoveries, and respectful of their predecessors, they have lost sight—and so the reader loses sight—of the framework of meaning and debate in which their documents truly become part of history.

THOMAS KUEHN
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CHARLES J. HALPERIN. *Russia and the Golden Horde: The Mongol Impact on Medieval Russian History*. Bloomington: Indiana University Press. 1985. Pp. ix, 180. $22.50.

The thirteenth-century Mongol conquest of Russia has long drawn the attention of historians, who have generally blamed the Mongols for all the ills that later emerged in Russian history. Autocracy, serfdom, mutilation, and much else has been attributed to the two-hundred-year Mongol dominion over Russia. The present study attempts a fresh reappraisal of a difficult theme. Charles J. Halperin, influenced by studies of Inner Asian history, argues that "nationalistic prejudices, cultural condescension, and scholarly ignorance have contributed to the continued neglect of the impact of the Golden Horde on Russian history," and he proposes to remedy the inattention by "using the paradigm of the medieval ethno-religious frontier as a comparative framework" (p. viii). Halperin then surveys medieval interaction between the Rus' settlements and various other peoples along the steppe and suggests a "fluid mosaic" of mutual benefit between the sedentary Slavic and nomadic Turkic peoples. He then examines the operation of the Golden Horde, its administration of Rus', the portrayal of the Mongols in medieval Russian sources, and the demographic, economic, political, and cultural results of Mongol rule.

Halperin does not mention, however, that the greatest problem is not prejudice, condescension, or ignorance, although each deserves some blame for current conceptions of Russo-Mongol relations, but a lack of sources. Almost no primary materials survive from Mongol administrators in Rus', and the Rus' texts themselves identify only the depredations of the Golden Horde. Halperin somewhat surprisingly proposes to make a virtue out of necessity, arguing that Russian bookmen consciously and consistently adopted an "ideology of silence" in treating Mongol affairs in order to avoid admitting beneficial interaction between the Mongols and Rus'. Therefore, Halperin suggests that we "read between the lines" so as to enlarge our perception of Russo-Mongol interaction. Such a technique is hazardous at best; at worst it resembles an argument from silence and obliges Halperin regularly to adopt the subjunctive mood and qualify his conclusions with "probably" or "perhaps." The result is far from satisfying. Even when treating the primary sources, Halperin assumes that all medieval Russian bookmen for more than two centuries uniformly adopted a hostile "ideology of silence" toward the steppe peoples and that the sources therefore consistently camouflage cultural interaction. But he nowhere introduces any proof for this counterintuitive proposition.

Halperin's findings constitute much less than the "pathbreaking reinterpretation" advertised on the dust jacket. He agrees that the conquest was a "catastrophe," but one without permanent effects. He lauds Mongol contributions to Russo-Asian trade; on balance, the Mongols did assist Moscow in its struggle with rivals, but he correctly attributes most of the credit to Moscow itself. Mongol institutions had some influence on Muscovite governmental and commercial institutions, but Halperin points out how thin was the veneer of Mongol influence, especially on Muscovite culture (the latter theme receiving a total of five and a half pages).

The greatest virtue of the book is the literate introduction of a novel perspective on Russo-Mongol affairs. His brief survey of Mongol influence, however, makes clear that the Mongol conquest did little to shape Muscovite society and culture.

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MODERN EUROPE

LEWIS W. SPITZ. *The Protestant Reformation, 1517–1559.* (Rise of Modern Europe.) New York: Harper and Row. 1985. Pp. xiii, 444. $22.95.

When the early volumes of the Langer series were published back in the 1930s, few historians doubted the usefulness of synoptic overviews of chronologically defined periods or the ability of scholars to meet the editor's demand for a "reliable survey" for general readers and students. But after nearly fifty years of aggressive scholarship, much of it debunking old myths, all of it illustrating the infinite complexity of things, these convictions are no longer widely held. Not many of us would assert now that our knowledge justifies confident generalizations. Nor are we so sure that it helps our students to be made to believe that the meaning of events becomes clear in the historical retrospect, that firm links can be demonstrated between actions and antecedent ideas and intentions, and that men are in control of the events they set in motion. There may be, as has been claimed, a revived interest in historical narrative. If this is so, our taste for sequential storytelling has been educated to recognize the difference between historical truth and authorial voice.

Lewis W. Spitz's coherent, thematically unified account of the Reformation therefore tells us as much about the historian as it does about the history of early Protestantism. The two objectives of this latest book by one of our leading Reformation historians are easily detected. As a teaching text, it shares the features of the series of which it is the long-delayed final volume: a core of solid chapters devoted to a dominant phenomenon, in this case the Protestant Reformation, is framed by augmenting sections describing the condition of Europe during the period in question. The tightly written narrative demands sustained attention from readers with some historical experience and a good deal of mental sophistication: it is not a book for beginners. These are the book's formal characteristics. But its underlying purpose is polemical: it seeks to return our understanding of the Protestant Reformation to the pivot from which recent historiography has removed it, the person and work of Martin Luther; and it tries by way of demonstration to rehabilitate the writing of history as the story of "uncommon men, the world's most persistent minority group," against the depredations of social historians with their preference for history "from the bottom up."

This ideological aim makes Spitz's book interesting for the professional as well as for the lay person. Except for a few disputatious digressions (swipes at "naive historians" wedded to quantitative methods, "lesser western historians" sympathetic to Marxism, partisans of *mentalités*), it is very good reading. Spitz is a purposeful guide through the intricacies of Reformation theology and ecclesiology. The biographical emphasis he favors offers satisfying detail about salient personages and vivid brief characterizations of a host of minor figures: Catholic controversialists, Spanish conquistadors, and many others. He concentrates dramatic moments into fine set pieces: Worms in 1521, the Marburg Colloquy, the execution of Thomas More, and many more. His explanations of the theological points at issue among contending reformers and religious parties are a model of clarity. Above all, he is a vigorous crusader for what he evidently takes to be an embattled position in his profession.

Two general attitudes seem to define this position. The first is a largely benign view of society, especially of relations among groups and individuals in early modern Europe. The word "class" does not occur in Spitz's pages except to deny that the thing itself, and the concept of it, existed. "There were no social classes in the modern sense of the word," he writes, "but rather estates or positions in society, and there was an impressive concern on the part of all, whatever their station in life, for the common good." Patricians, he says, "ruled without remuneration . . . and were much concerned with the common good." No analysis is given to divergent perceptions of "common good" or to the many other questions begged in these and similar sentences. The work of Heinz Schilling, Thomas Brady, Robert Scribner, Rudolf Endres, Winfried Schulze, David Sabean, and other recent students of the political functioning of early modern German society goes mostly unacknowledged.

Second, Spitz's position depends on his confidence in the historical effectiveness of ideas via the intellectual influence exerted by great men and the books they write. The very last words in his volume, a quotation from Luther, tell us that "every great book is an action, and every great action is a book." Spitz acknowledges "societal forces" as factors in the attempt to explain events. But it is the purposeful actions of men, always prominent men, that receive the lion's share of exposition, and these actions are shown to have followed plausibly, sometimes logically, on a prior sequence of ideas and volition. Spitz is an intellectual historian of the old school, unreconstructed in his priorities and frequently dismissive of the efforts of other scholars to reveal hidden strands in the thick texture of historical reality.

These two attitudes govern Spitz's interpretation of the Reformation as "the most characteristic and dominant achievement of the time." It was, first and foremost, "a movement of the human spirit," the transcendent origin of which—"Fire fell from heaven"—is emphatically conveyed to the reader. In its manifestations in the mundane arena, the Refor-

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mation is seen in terms of personal actions. It "began when a very personal matter, Luther's struggle for the right relationship to God, became a popular cause." It was "born deep within a single individual." No less personal was the response Luther and the other reformers evoked. Their evangelical message—salvation by grace alone, guidance by Scripture alone—struck root in the hearts of individuals in high and low places, and this "groundswell" in turn encouraged those in positions of power to act against pope and church. Religion was on all levels the primary motive. People were "deeply involved in the religious struggle," Spitz says, but he does not pause to analyze the intricate relationships between religion and immediate worldly interests. He affirms the old view that Germans were more intense about "religious enlightenment" than were other nationals. German humanists "tended to be more serious and devout" than Italians. In judging the new religious arrangements issuing from this spiritual ferment, Spitz uses established mainstream Protestantism as a standard. He is not very sympathetic to Anabaptists and other sectarians, making considerable play of the Münster episode and disparaging Thomas Müntzer. "Anabaptism," he concludes in a sentence that could have been written by Norman Cohn, "appealed for the most part to the poor, the weak, the disaffected, and resentful." The Reformation was "radical" in its ultimate effects—religious pluralism, national churches, a boost given to such long-range trends as rationalism and overseas expansion—but conservative in its immediate impact in that it left essentially unchanged both the human scene, with its intractable structures and institutions, and the supernatural world of angels, devils, expectations of Doomsday, and pessimism concerning human effort. This double focus on delayed and direct results blunts somewhat the point of Ranke's verdict on the Reformation, which is quoted with approval, as "the most profound spiritual revolution ever experienced by a people in so short a period of time."

Spitz's book is likely to win favor with the present generation of college students who will be pleased by its upbeat tone and its idealism (in both senses of the word) while also learning a lot from its genially presented wealth of information. His colleagues may wish that Spitz had used his unmatched knowledge of the period in less conventional ways to advance our understanding of the subtle social mechanisms by which history operates. Historians sympathetic to this other approach have been making gratifying progress in Reformation scholarship. It is too bad that Spitz is not on their side.

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MARY JO MAYNES. *Schooling in Western Europe: A Social History.* (SUNY Series on Interdisciplinary Perspectives in Social History.) Albany: State University of New York Press. 1985. Pp. ix, 177.

Have European primary schools functioned since the nineteenth century as "implements of a new style of class domination" (p. 150)? Answers to the question are reviewed in this useful synthesis of monographic literature published since the 1960s on the history of primary schools in England, France, and, to a lesser extent, Germany between the late eighteenth and late nineteenth centuries. Much of this work has been explicitly "revisionist" in its shift from the political and administrative emphasis of older educational histories to a focus on the social and economic history of schooling. Revisionists also reject the Whiggish assumption that nineteenth-century expansion of public schooling typically had a progressive impact on individuals and nations. Instead, they stress the "repressive" aspects of compulsory education. Author of a recent monograph comparing primary education in Baden and in the Vaucluse in southeastern France between 1750 and 1850, Mary Jo Maynes is emphatically a revisionist.

Many insights of the newer educational histories are challenging and stimulating. As François Furet and Jacques Ozouf's *Lire et écrire* (1977) demonstrated, many northern French localities displayed high literacy rates before primary education was promoted by the Guizot Law of 1833 or the Ferry Laws of 1881–82, laws now viewed as following popular demand for literacy rather than stimulating it. Industrialization, long assumed to be a catalyst for educational improvement, actually coincided with declining literacy in both Lancashire and the French department of the Nord. In areas where popular interest in schooling lagged during the early nineteenth century because literacy and computational skills had no immediate practical value, parents often resented and sabotaged educators' efforts to divert youngsters from labor in the field or workshop, which seemed essential to the family economy. Maynes also believes that older "voluntary" means used by English and French artisans to acquire literacy provided a "creative radicalism" (p. 108) preferable to the institutional offerings imposed by elites eager to destroy the workers' distinctive culture. Because most people, including workers, eventually accepted and even welcomed public schools, the working class lacked "institutions in which a genuine culture of opposition could flourish" (p. 114).

Of course, industrial, commercial, bureaucratic, professional, and religious elites did not always agree about popular schooling. Maynes draws sharp national contrasts between an English "obscurantist"

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aristocratic elite that resisted public education longer because of the fear of schooling's effect on the masses (p. 54); a French "bourgeois" elite that was able to impose a Gramscian model of "hegemony" (p. 147) on the masses, which internalized the liberal credo that meritorious scholastic performance could promote individual advancement; and German ruling classes that "relied much more heavily on the tactics of coercion and repression," the German states' early leadership in mass literacy having resulted more from religious goals and "enlightened despotism than [from] liberal hegemony" (p. 149).

Sound generalizations require support from empirical data, and not all revisionist studies have yet provided adequate documentation. Maynes acknowledges both the need for studies of "how much employers valued schooling" (p. 131) and the scarcity of "evidence about the character of experiences at school" (p. 150). While conceding that studies proving or disproving a link between schooling and jobs held later "are rare" and, thus, that "any generalization on this point would be premature," she nonetheless generalizes that "the few studies" of North American and French locales "suggest that during the early phases of industrialization formal education had relatively little impact on what jobs school graduates went into or how well they did" (p. 131). Yet footnote 33, which accompanies this statement, partially contradicts the generalization because Maynes there admits that, unlike Harvey Graff's Canadian study, the French studies cited—those of Patrick Harrigan and Robert Gildea—actually point to "a moderate degree of social movement attributable to schooling alone."

Thus, more than one controversy about the history of European education still awaits resolution. Maynes provides a clear and informative introduction to recent controversies. Minor flaws are occasional typographical and proofreading errors, a lack of subject-verb agreement (p. 127), and, more seriously, the mistaken identification of "Villemot" as "Minister of Education" in 1887 (p. 100).

LINDA L. CLARK
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ALBERT VAN HELDEN. *Measuring the Universe: Cosmic Dimensions from Aristarchus to Halley*. Chicago: University of Chicago Press. 1985. Pp. viii, 203. $30.00.

I will not find it easy to express all of the praise that this fine book deserves. It will be read, I suspect, primarily by historians of science. What a pity that other historians will deny themselves participation in one of the great intellectual adventures of mankind, which Albert Van Helden presents so expertly—the reluctant recognition that the world in which we live is not finite and manageable but for all intents and purposes infinite. During the Middle Ages, scholastics liked to calculate how much longer Adam would have had to walk if he had set out for the sphere of the fixed stars on the day of creation and covered twenty-five miles a day. By the end of the seventeenth century one Francis Roberts remarked that light, the velocity of which was known with reasonable accuracy, would take more time to reach us from the nearest stars than a ship would require to sail to the West Indies, and Roberts was placing the stars considerably closer than the best astronomers did by that time. During the seventeenth century the world, which had seemed adjusted to the human scale, transformed itself into a place no less strange to the average person than the four-dimensional universe of Einstein is to most of us today. Its enormous expansion was not the whole of the change by any means, but it may well have been the aspect that most readily pricked the imagination. As Van Helden's title suggests, this is the story of the replacement of assumptions, justified by questionable observations and accepted largely because they were comfortable, by scientific measurements made possible by the telescope. The book is thus filled with numbers and considerable technical detail, which are, however, lucidly presented and not so difficult that anyone should be daunted. What they add up to is an excellent account of one of the fundamental changes that ushered in the modern intellect.

Van Helden starts with the Greeks and their efforts to measure the universe, indicating the inevitable weaknesses of their methods and the almost universal acceptance of their results because people could live with them. The Greek dimensions of the world became part of the general heritage of educated people. Copernicus scarcely altered them, although Kepler, in this matter as in everything, was more revolutionary. Nevertheless, it was primarily the telescope, which entered scientific life in the first decade of the seventeenth century, that introduced the possibility of rigorous measurements and thus refashioned our conception of the world in which we live. But the telescope did not perform this feat instantly. It took more than a generation for astronomers to seize the potential of their new instrument and to prepare their minds for what it could reveal. The first good measurement of the size of a planet as seen from the earth, by Gassendi in 1631, which revealed that the visual size of Mercury could only be a fraction of what everyone had accepted and thus raised the possibility that the solar system was far larger than people had believed, was greeted with incredulity, not least by Gassendi himself.

One of the fascinating features of the book is its set of tables of distances assigned to the planets by successive astronomers. As the telescope gradually

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revealed a universe of radically different size, almost no one erred in placing the planets too far away. Astronomers slowly revised their estimates upward, as new measurements forced them to do so, and our present dimensions were approached asymptotically from the underside. Although a measurement of the distance of the sun that we regard as reasonably accurate had to wait until the mid-eighteenth century, scientists had arrived at a good understanding of the size of the solar system by the end of the seventeenth; they remained, however, reluctant to believe that the stars could be as far away as we now know them to be. Histories of early modern astronomy have generally concentrated on the heliocentric system. Van Helden's excellent book presents another story, and one no less fraught with significance. I cannot recommend the book too highly.

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FREDERIC LAWRENCE HOLMES. *Lavoisier and the Chemistry of Life: An Exploration of Scientific Creativity.* (Wisconsin Publications in the History of Science and Medicine, number 4.) Madison: University of Wisconsin Press. 1985. Pp. xxiv, 565. $38.50.

On a cold morning in February 1783, a guinea pig was placed on cotton wool in a makeshift iron basket, which was fitted inside a contraption containing ice and well insulated from the air outside. During many hours in this inhospitable environment the animal continued to breathe and give out body heat, thereby melting some of the ice. The resultant water was drained off and weighed, thus giving an index of the amount of heat produced by the simple process of breathing (or so it was thought). This turned out to be roughly the same as that produced by a candle burning to form the same quantity of gaseous product ("fixed air").

From experiments of this kind the great French chemist A. L. Lavoisier (1743–94) derived support for a growing conviction that respiration and combustion were intimately connected. He intended to initiate a "chemical revolution" (so his private papers reveal), and later historical judgments would not have disappointed him. Famous above all for his part in the overthrow of the phlogiston theory, the discovery and naming of oxygen, and the clarification of our understanding of a chemical element, he was equally interested in animal and plant phenomena and in physical and chemical processes. Indeed, the two realms were connected through their participation in the uptake and release of air.

This is the thesis of the distinguished historian of biochemistry Frederic Lawrence Holmes in what must rank as an epoch-making work on this titan of science. Several features of the book differentiate it from previous essays on Lavoisier. In 502 pages of text the author gives with single-minded dedication an exposition of his stated theme, rarely deviating into more familiar areas of the chemical revolution. More than his predecessors, Holmes concentrates on Lavoisier's unpublished manuscripts; he is acutely aware of the gulf between polished public statements and the patchwork of conflicting ideas that is the essence of much creative scientific thinking. Retracing the mental processes by which Lavoisier responded to his "grand analogy" between burning and breathing, he cites generalizations of Karl Popper, Thomas Kuhn, Michael Polanyi, and others, testing them by the facts of Lavoisier's case. Again and again it is in private notebooks and memoranda of his investigations that new and valuable clues are found.

Such experiments, endlessly repeated and varied, were once the very stuff of the history of science. The supreme task of the historian was to chart a way through the labyrinth of scientific investigations in order to understand how one led to another and to disclose the assumptions, attitudes, and ambitions that underlay them. Now, however, priorities have shifted, though not always in a welcome direction. Holmes observes: "Within the newer movement toward a social history of science, investigations are usually treated only at the surfaces where the social dimension is assumed to impinge upon their direction and character. Yet it is the investigations themselves which are at the heart of the life of an active experimental scientist. . . . For him the social context is peripheral unless it becomes an impediment to his ongoing work. If we are to understand scientific activity at its core, we must immerse ourselves as fully as possible into those investigative operations" (p. xvi). The whole book exemplifies the wisdom of this approach and will be invaluable for historians of physics, chemistry, and biology. For all aspirants to the profession, and not a few veterans, Holmes's introduction, at very least, should be compulsory reading.

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ROBERT BROWN. *The Nature of Social Laws: Machiavelli to Mill.* New York: Cambridge University Press. 1984. Pp. ix, 270. $39.50.

Despite the title of this work, Robert Brown's essay does not limit itself to a discussion of the philosophical structure of social generalizations. Rather, the author attempts nothing less than the history of social laws of all descriptions—sociological, psychological, and economic—formulated from the sixteenth to the nineteenth centuries, together with an analysis of their development. Indeed, the sheer

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magnitude of this task almost certainly accounts for the book's greatest weakness. It is clearly impossible to do justice to such a vast subject within the confines of less than three hundred pages. Consequently, the effect cannot but occasionally strike the reader as somewhat superficial. Brown seldom devotes more than a few pages—often no more than a few paragraphs—to any one thinker. Thus, Machiavelli, whose name appears in the subtitle of Brown's monograph, is treated in two or three pages, and then primarily as a student of society whose interests emerged from his concern for practical politics. And where a major social philosopher is not dealt with cursorily, he is often not discussed at all. For example, Voltaire, Diderot and the other Encyclopedists, together with Rousseau, Constant, and Tocqueville—to take only a few French thinkers—are not mentioned.

Notwithstanding the serious weaknesses that stem from the limitations of space, this is often an interesting and enlightening work. We are informed, for example, that adherents of natural law contributed far less to the development of social Newtonianism than did the early economists, whose concern for causal regularities eventually gave shape to the modern concept of social laws. Brown's argument here seems a sound one inasmuch as natural law theorists were as much interested in making normative judgments about society as about the nature of empirical claims respecting human actions. Brown is in error, however, in asserting that, when Montesquieu predicates the existence of social laws, these are to be taken purely as laws of moral obligation that men may or may not obey. In fact, Montesquieu appears to have concluded that natural laws not only provide a framework within which men are obliged to direct their actions but also serve as generalizations respecting how men for the most part do behave. That these generalizations permit of some exceptions does not deny them the status of social laws, any more than certain variations in the form social arrangements may take will deny them the status of being consistent with natural law.

The analysis Brown offers of John Stuart Mill's discussion of the nature of social laws is particularly valuable, especially that section that treats Mill's concerns respecting the relation between generality and precision. Brown's sympathies clearly lie with those writers who have questioned our ability to construct scientific laws of society using the laws of physics as a model. And nowhere are Brown's arguments more telling than in his criticism of Mill's account of the proper structure of a true social science. Brown's discussion of Mill's chimerical science of society is the strongest element in this work. The reader can only regret that the same attention and detail devoted to Mill's views—and to those of Comte before him—were not extended to a few of the other seminal figures whom Brown chose to examine. Had Brown done so, the result would almost certainly have turned an interesting book into a highly valuable one.

RONALD HAMOWY
University of Alberta

MICHAEL MOONEY. *Vico in the Tradition of Rhetoric*. Princeton: Princeton University Press. 1985. Pp. xxiv, 318. $26.00.

Interest in Giambattista Vico in the English-speaking world has flourished in the last twenty years. Philosophers and historians of ideas have come to recognize a significance in his theories of history and society that his contemporaries (so it is often assumed) failed to notice. Yet this belated recognition of a seminal thinker has been bought at a price; far too frequently Vico is interpreted in the idiom of a later development in the history of ideas, leaving the detailed explanation of the genesis of his ideas uncertain and problematic. This is precisely the dilemma that Michael Mooney has sought to remedy. He has set himself the task of rescuing Vico from the philosophers, with their preoccupation with the *verum/factum* principle as an epistemological criterion, looking instead to the classical and Renaissance sources that so decisively influenced the cast of Vico's thought. And his treatment of the detailed themes within a complex tradition is clear and assured.

Mooney traces the source of Vico's ideas back to the very beginnings of the rhetorical tradition, in Greek and Roman accounts of the role of rhetoric and its relation to philosophy. Where Plato had emphasized the gulf between philosophy conceived as the pursuit of truth and rhetoric as the art of persuasion, Mooney is interested above all in rhetoric as a synthesis of practical wisdom. It is Cicero's depiction of the ideal orator as the embodiment of eloquence and wisdom that dominates these pages, serving as a standard to evaluate the contrasting formulations within the rhetorical tradition. Mooney charts the history of rhetoric from its classical ideal through the more specific and practical conceptions of its role in the medieval world to the triumph of Renaissance humanism and beyond. Much of this will be familiar to specialists in rhetoric, but Mooney has done a service to Vico scholarship in focusing attention on a tradition of thought that has too often been either ignored or treated in cavalier fashion. Indeed, Mooney's account of the fortune of rhetoric in the age of Peter Ramus and René Descartes serves to highlight the significance of an earlier rhetorical ideal in Vico's critique of the insidious influence of "analysis" in the civic culture of his day.

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All this is skillfully done. And, in reading the *Institutiones oratoriae* and *De nostri temporis studiorum ratione* in the context of the rhetorical tradition, Mooney has shed light on aspects of Vico's argument that have eluded philosophers concerned primarily with epistemological issues. He is right, too, in seeing the *Scienza nuova* as a philosophical elaboration of notions first mooted in humanist scholarship. But he is less sure in explaining precisely how Vico made the leap from the world of philological research to a science of society. Nor is the omission in his account far to seek. Vico had crucially enlarged the scope of his theories in the *Diritto universale,* and, although Mooney has a sound grasp of the importance of jurisprudence for the development of Vico's ideas and refers to Vico's writings on law, he does not subject the *Diritto universale* to close textual analysis.

Yet this book remains a most welcome addition to the Vico literature. We can but hope that Mooney will proceed to the study of law and religion in Vico (which he promises us in his preface) without delay.

B. A. HADDOCK
University College of Swansea

MARÍA DEL CARMEN IGLESIAS. *El pensamiento de Montesquieu: Política y ciencia natural.* (Alianza Universidad, number 381.) Madrid: Alianza. 1984. Pp. 409.

María del Carmen Iglesias provides another contribution to the recent Montesquieu scholarship of Robert Shackleton, Charles Beyer, and others. Her book, which includes a preface and five chapters divided into twenty-four sections, covers Montesquieu's formative years; the influence of Descartes, Spinoza, and Leibnitz and of Newtonian experimental philosophy; and Montesquieu's ideas on nature and life sciences, the naturalist crystallization in *The Spirit of the Laws.* It ends with a conclusion on antiauthoritarian equilibrium vis-à-vis uniformity.

Carmen Iglesias studies the paths to which Montesquieu resorted in reaching his judgments. Paralleling the development of the method in the physical and natural sciences, Montesquieu's well-furnished mind arrived at knowledgeable conceptions, permitting him to analyze successfully facts, deeds, institutions, and values of social and political reality. This book follows Montesquieu's objectives, recognizing in his study of the crystallization of scientific methods those of his predecessors and contemporaries in the seventeenth and eighteenth centuries. The naturalism to which these methods were applied became the idea one had of it and related epistemologically to its own methodology. The conception or conceptions that this epoch had of naturality—explicitly at some levels, but implicitly as part of an incomplete formation of ampler views, at other levels—resulted from partial investigations. This permitted the investigations' piecemeal fundamentalism.

In her most penetrating and informative chapter, Carmen Iglesias sees Montesquieu establishing for a French constitutional monarchy an antiauthoritarian equilibrium opposed to existing monarchical uniformity. She compares dynamic balance of the English constitution's constant action-reaction to movement of the Newtonian physical world. To Newton's providential God maintaining the universe corresponds active, rational man, vigilant of his own creations, capable of inventing rules. Indeed, the main characteristic of human nature is man's ability to invent and apply rules, codes, and norms that are always changing and varied, mostly responsive to his environment's exigencies, yet contributing to its transformation. Such are rules required by the very nature of things: by that physical nature that is studied on analysis of fossils' origins. Thus, to attain a certain maturity human nature ever demands measure, limits, selection, slow development; this nature, equal in all men, is as much respective of their rational possibilities as of their sentiments and passions.

To prevent a minority's abuses from consigning all others to slavery, Montesquieu propounds a moderate formula of social and political coexistence in that "power curbs power." This formula, seemingly easy to discuss but difficult to practice, is indispensable to protect citizens from despotic ambitions. It derives a new interpretation, related to the "happy medium"—that measure, constituting with freedom, one of the keys to Montesquieu's political thought.

It is not chance, happenstance, or a single accidental episode that governs the world, as Montesquieu wrote in a well-known text, but rather some general, moral, and physical causes, which include fortuitous events.

Montesquieu passionately rejects despotism; his lucid though not despairing skepticism entails a knowledge of human fragility that does not paralyze action but instead makes it valiant and irreplaceable.

ALFRED J. BINGHAM
University of Maryland

DONALD W. LIVINGSTON. *Hume's Philosophy of Common Life.* Chicago: University of Chicago Press. 1984. Pp. xiv, 371. $30.00.

The title of this book derives from Donald W. Livingston's contention that Hume accepted the order of "common life" as the necessary presupposition of all "true" philosophy. "True" philosophy cannot be autonomous because the philosopher can never stand outside the beliefs, customs, and preju-

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dices of the moral world. For Hume, then, "true philosophy is limited to the task of systematizing and rendering mutually coherent the established beliefs and principles of common life" (p. 273). Since he emphasizes the limited usefulness of philosophy for understanding reality and changing the way people think and live, his approach is decidedly skeptical, but it is "mitigated" rather than subversive skepticism. Although Livingston's Hume can be a devastating critic, the philosopher's harshest criticisms are directed against attempts to transcend the principles of "common life." The Cartesian who purports to sweep away all accepted beliefs in order to build a new philosophical system, the clergyman or philosopher who interprets human affairs in terms of a sacred or secular plan of history, and the political radical who would restructure society in accordance with abstract notions of justice are in Hume's eyes all guilty of the same sort of error, which Livingston terms "metaphysical rebellion."

The book's strongest chapters address the narrative structure of the moral world. For Hume, Livingston argues, narrative associations are fundamental. Truly timeless thinking is impossible because all human conceptions are rooted in "tensed" ideas. We are constantly comparing what is happening in the present with what we know or think has happened in the past, and on this basis we make predictions about what is to come. This process is as true of the physical world (for example, one billiard ball striking another) as of the moral world. From the standpoint of positivism, of course, knowledge of the moral world is vastly inferior, but Livingston's Hume is no positivist. On the contrary, he believes that moral philosophy is superior to natural philosophy by virtue of its ability to explain the actions of moral agents. Moral philosophy gains in intelligibility what it loses in predictive force. Since hindsight makes past actions more intelligible than present ones, it follows that "the moral philosopher giving moral accounts is essentially a historian" (p. 207). Hence, Hume's abiding concern with history, not only in the *History of England* but also in the many historical discussions elsewhere in his writings. In chapters on historical understanding and the correction of narrative judgments about the past, Livingston further explores the important role of history in Hume's moral philosophy.

This is very much a philosopher's book, and some historians of ideas may find it difficult going. They may also wish that the author had a firmer and more contextual grasp of the intellectual history of Hume's age. Nevertheless, Livingston includes much perceptive material on subjects that have received far too little attention from Hume scholars. Anyone with an interest in Hume's philosophy of history or the relationship of history and philosophy in Hume's writings will be grateful to Livingston for this challenging study.

RICHARD B. SHER
New Jersey Institute of Technology

EDITH WYSCHOGROD. *Spirit in Ashes: Hegel, Heidegger, and Man-Made Mass Death*. New Haven: Yale University Press. 1985. Pp. xvi, 247. $22.50.

According to a well-established tradition, which Edith Wyschogrod labels the "authenticity paradigm," not only are we expected to live well but we are also expected to die well, and the failure to do so casts a shadow over the whole of a man's life. If, however, we manage to die well, as Socrates did, this shows that we have lived a good life.

In this ambitious book Wyschogrod maintains that the traditional view is no longer applicable in an age of man-made mass death, of world wars and concentration camps; rather, in view of the impersonal, bureaucratic forms of death made possible by the new technology, we need a drastically new attitude toward the "death event." To help prepare us for this fundamental change, she offers an interpretation of what Hegel and Heidegger have said about human finitude. Approaching Hegel from his *Philosophy of Nature*, she offers some fresh insights into his conception of man's efforts to transcend nature, and this is the best part of her book. What she has to say about Heidegger, with whom she is more in sympathy, is less revealing, and her own contributions to a new perspective on the "death event" seem far too brief and at times opaque. One senses two books struggling to emerge from this study, one an exegesis of Hegel and Heidegger and the other an analysis of the horrors of the twentieth century. I find it more disconcerting than illuminating to encounter quotations from survivors of death camps mixed with Hegelian exegesis.

But do we really need an entirely new paradigm about the "death event"? After all, the question of how we die is ambiguous, referring either to the manner of our death (whether natural, man-made, or man-made with the new technology) or to the attitude we take toward our death. I am not persuaded that the new technology requires a fundamental change in the attitude we take. Wyschogrod is impressed that some victims in concentration camps die badly, without dignity and deprived of their humanity. Should we treat inmates of concentration camps as being already dead, a possibility seriously considered by Wyschogrod, or, more reasonably, should we simply acknowledge that the circumstances of life in a concentration camp may annul or seriously qualify human agency and responsibility? Maybe we only really need to be less impressed by the failure to die well. Imagine Socra-

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tes transported in time to a modern concentration camp where, breaking under torture, he proceeds to recant the life of reason, to name his friends as co-conspirators, and to plead for exile to California. Could anyone seriously maintain that this would indicate that Socrates had not lived a good life? Perhaps the new man-made mass death should only convince us to think less about dying and more about how to live meaningfully under the threat of impending disaster.

BURLEIGH T. WILKINS
University of California, Santa Barbara

JACQUES DROZ. *Histoire de l'antifascisme en Europe, 1923–1939.* (Textes à l'Appui.) Paris: La Découverte, with the cooperation of the Centre National des Lettres, Paris. 1985. Pp. 318. 125 fr.

Someone said that the way to define fascism is to write its history. Jacques Droz wants to define antifascism in the same way. He brings to this survey of European antifascism before the Second World War a vast reading in secondary literature and a talent for combining conceptualization and careful detail. Told is the story of a losing cause, followed nation by nation in successive chapters on Italy, Germany, Austria, France, Great Britain, and Spain. Much is known about fascism in these countries, Droz remarks, but not enough about its enemies. His purpose is to draw all of them together into the first general history of European antifascism, a synthesis designed to orient further research. This is a tall order. Scholars have recognized the variety of European fascisms, but they haven't seen anything yet. Surveyed here are antifascisms in profusion: different Left, Right, and Center, different in every country, in every political party, and, seemingly, in the mind of every major thinker. Against fascism, the imagination took power—and all was lost. "Whoever does not know how fascism works," a writer warned, "will become its victim" (p. 98). Droz concurs. His theme is that the failure of antifascists in this period began in a failure of perception, a collective blindness to the demonic nature of fascism, and resulted in an incapacity to combine effectively against it.

In describing this land of the blind, Droz sees few political factions in a very good light. Conservatives in the German Resistance, for example, opposed the paganism and amorality of Nazism, but too many, he explains, identified with its hatred of communism, its authoritarianism, and its impulse toward expansion. Thus, Resistance leader Karl Goerdeler in 1938 advocated German possession of the Sudetenland, Danzig, and the lost colonies. Such conservative conspirators against Hitler, Droz argues, did not so much fear Nazism as something worse: that Nazism would lead to bolshevism in Germany. Most of his book, however, is concerned with the paralysis of the European Left. Scarred by the First World War, workers in the democracies, Droz observes, feared rearmament as much as fascism, and sometimes more. To this bitter memory of one war were added bitter memories of another, the conflict between the Second and Third Internationals, turning working-class organizations not on the fascists but on each other. Communists and socialists forgot nothing and forgave nothing, and every new attempt to unite against fascism became another contest to settle old scores.

Such are some themes of this valuable work. Missing is consideration of the silent antifascism—or afascism—deep in the population, the dull, mass aversion to fascism that was the real obstacle to its deeper penetration of European society. But Droz does enough. Frankly, the nature of fascism is probably as confusing to historians today as it was to the men (there are few women) in this book. Droz, however, makes the nature of antifascism much clearer.

GILBERT ALLARDYCE
University of New Brunswick

JOSEF W. KONVITZ. *The Urban Millennium: The City-Building Process from the Early Middle Ages to the Present.* Carbondale: Southern Illinois University Press. 1985. Pp. xxi, 265. $24.95.

This book is a short analysis of a large and difficult subject: how Western man has perceived and built cities since the urban revival in the Middle Ages. In view of the paucity of scholarly work on city building and the subject's immense dimensions, Josef W. Konvitz aims at nothing more than a modest essay.

Given the totality of urban destruction after the fall of Rome, one can only marvel at the achievement of Europeans in reestablishing the tradition of city building after the lapse of many centuries. The uninterrupted growth of cities since medieval times, the constant tendency of cities to grow at a pace exceeding the total population, and the inability of alarmed urban magistrates to check population growth evoke both admiration and alarm. The author reminds us that even in the fourteenth century, when conditions seemed to favor rural growth over urban, cities nevertheless continued to grow. Konvitz asserts that catastrophe "only enhanced the attractiveness of urban living" (p. 11).

In his analysis of the city-building process, the author divides the past millennium into three periods, or "modes," that provide the basic framework of the book. Konvitz maintains that at any given point in the last millennium any or all of the three

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modes may be present; they vary only in relative emphasis. The first, which he labels the cultural mode, extended from the start of the medieval revival of cities to the late seventeenth century. At this stage the emphasis was always on the user: the urban environment was to be intelligible and readily available to all. Building scales were for the most part small, public attention was focused on urban affairs, and the urban environment was easily and inexpensively adaptable to the new uses. The investment required by the community before construction was minimal.

The cultural mode gave way to the economic mode in the eighteenth century. Emphasis shifted from the user to the investor; everything was subordinated to making the urban environment a place for production and exchange. The simple construction that had prevailed in the cultural mode now gave way to far more elaborate and far riskier structures built in a new speculative environment. The third of Konvitz's three modes came into play late in the nineteenth century, when the cost and complexity of urban development became so enormous that individuals could no longer control them. The task was therefore taken over by regulatory bodies, and the control of city space became a vital political and social issue.

Interspersed in Konvitz's analysis of the city-building process is a wealth of interesting and original information on how cities come into being. Often, the reader's first impulse is to question the relevance of the facts presented, but his doubts are removed as he reads on. The subjects covered range from modern container terminals to medieval clock towers and from the obsolescence of urban infrastructures to tenth-century monasteries. Readers interested in urban cartography will be grateful for an original and refreshing chapter on the appearance of the first scientific city maps in the eighteenth century. This book is clearly a valuable addition to the bibliography of urban history.

L. L. BERNARD
University of Notre Dame

THOMAS ANDREW GREEN. *Verdict according to Conscience: Perspectives on the English Criminal Trial Jury, 1200–1800.* Chicago: University of Chicago Press. 1985. Pp. xx, 409. $34.00.

A definitive history of the English criminal trial jury cannot yet be written. So Thomas Andrew Green's book provides us instead with a thoroughly researched and abundantly footnoted (about a quarter of the total contents) progress report on the current state of research on the subject. Although noting that in modern times most emphasis has been placed on the importance of the jury as a "bulwark against tyranny," he himself believes that "behaviorally the most significant aspect of the jury's place in medieval and early modern English culture . . . [has been its] role as a mitigator of capital sanctions in felony trials" (p. 379).

Part 1 of the book deals with the criminal jury's late medieval origins and how it grappled with the moral challenge of making a life or death decision about a neighbor's guilt or innocence once the church would no longer allow that to be demonstrated unequivocally by God through the ordeal process. Part 2, which covers events during the Tudor and Stuart periods, takes up nearly half of the total text. Green discusses the effect on jurors of having to rely more on evidence presented in court and less on their own personal knowledge of the case and of the persons involved in it. Their task was not made any easier by knowing that judges, and sometimes even the fearsome Court of Star Chamber, were prepared to coerce jurors whom they considered to have acquitted prisoners in spite of evident proof of their guilt. By the late seventeenth century, however, the decision in *Bushell's Case* (1670) had effectively ended any official coercion of jurymen. Meanwhile, some political and religious controversialists began to assert that jurors in some instances actually had the right to determine questions of law. In part 3 Green tells how in the Age of Reason rational as well as religious considerations influenced jurors' decisions about who deserved to suffer death for having committed a felonious act. Green also notes that Fox's Libel Act (1792) did at last give jurors, in one important regard at least, the power to decide law as well as fact. And, finally, the author records how the criminal law reforms of the early nineteenth century, by making the punishments more suitable to the crimes and by removing the death penalty from most felonies, made the task of arriving at just verdicts much less troublesome to tender consciences and reasoning minds.

This book contains a great deal of repetition or, at least, recapitulation; some parts are a little less impressively persuasive than others. Specialists in English legal history, however, will on the whole find it both interesting and valuable.

MICHAEL DE L. LANDON
University of Mississippi

CHRISTINA LARNER. *Witchcraft and Religion: The Politics of Popular Belief.* Edited by ALAN MACFARLANE. New York: Basil Blackwell. 1984. Pp. ix, 172. $24.95.

Christina Larner was under fifty years old when she died in 1983. Her death deprived the academic profession of a first-rate intelligence whose work is of equal interest to both of her chosen disciplines,

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history and sociology. This volume contains a collection of articles, papers, and lectures, some previously published, tactfully and minimally edited by Alan Macfarlane. They begin with the piece on James VI and witchcraft with which she first made her mark in 1973 and conclude with the Gifford Lectures in Natural Theology, which she delivered at the University of Glasgow in 1982. There is, inevitably, some overlap and repetition, but everything is worth reading, and everything is clearly written and witty—"in *Times* English," as she herself put it.

Larner established herself as the leading authority on Scottish witchcraft with her book *Enemies of God*, published in 1981. In the years before her death she was moving on to a consideration of witchcraft as a European phenomenon. She concluded that the witchcraft mania required certain preconditions: a predominantly rural economy, a peasantry that believed in the power of witches, and an upper class that believed in the power of the devil. What produced the mania in early modern Europe was, first, the Christianization of rural society, which, Larner argued, following Jean Delumeau, occurred during the age of the Reformation and Counter Reformation, the true "age of faith" in European history. The second factor was the expansion of the state's control of the machinery of justice and a change in the character of the justice it administered, from compensatory to retributive. The end of the *lex talionis* also was crucial: if wrongful accusation was no longer punished, the door was open to all sorts of speculative charges. The sanctification of the state with the development of the doctrine of the divine right of kings was also vital. "The witchcraft prosecutions in Europe," she wrote, "were exactly coterminous with the period during which Christianity was a political ideology" (p. 90).

Larner was uneasy about the hypothesis that the mania was part of a sex war. Unlike infanticide, the other crime with which large numbers of women were charged, witchcraft was sex related but not sex specific. Women were prosecuted in such numbers, she suggests, because "in the law and order crises generated by the new regimes of early modern Europe women were a prime symbol of disorder" (p. 86). Witches were the enemies of God; that 80 percent of them were female "could have added fuel to the misogyny of the age rather than be a direct consequence of it" (p. 87). She regarded modern "witches" as risible; their publications "all look as though a copy of *Penthouse* had been crossed with a PhD thesis" (p. 83). She dismissed Margaret Murray's "pagan religion" hypothesis as lacking in evidence and was dubious about the "Protestant deprivation" theory of Keith Thomas and the "social strain" hypothesis of Thomas and Macfarlane. The mania ended because of what Larner called the "political dethronement of God" rather than because of the "rise of modern scientific thinking, rationalism, and the Enlightenment" (p. 139).

The reader closes this fascinating book with a profound sense of regret. What might its author have accomplished in future had she been spared?

MAURICE LEE, JR.
Rutgers University

KENNETH R. ANDREWS. *Trade, Plunder, and Settlement: Maritime Enterprise and the Genesis of the British Empire, 1480–1630*. New York: Cambridge University Press. 1984. Pp. ix, 394. Cloth $49.50, paper $16.95.

Few historians have consistently explored the important area that falls between the interests of economic historians, preoccupied with trade and commerce, and those of naval historians, obsessed with the emergence of British naval supremacy. British scholarship since the war has been graced by two giants in this field: Kenneth R. Andrews and David B. Quinn. The former signaled his arrival in 1951 with a doctoral thesis on Elizabethan privateering, which has been followed by a flood of books and articles charting the tentative efforts of Englishmen to grab a slice of the new trades and territories opened by the Iberian powers in the fifteenth and early sixteenth centuries. The present book, building on this long experience, provides a brilliant synthesis of English maritime endeavor between 1480 and 1630.

After an excellent introduction, which discusses the context, nature, and motive of English maritime enterprise, fourteen chapters cover different spheres of activity in detail, including the establishment of trade with Muscovy and the Levant, the sea war of 1585–1603, and transatlantic developments, which occupy nine chapters. After a bright opening in the later fifteenth century, centered on Bristol's support of the Cabots, English enterprise in distant waters faltered for half a century. Thereafter, more difficult trading conditions in England's traditional European markets helped stimulate more distant voyages, but progress—especially in establishing permanent settlements overseas—was slow.

The motives of those involved, especially in transatlantic enterprise, were rarely simple; trade and plunder were closely linked in the minds of merchants and seafarers, although ideas of colonization took a back seat until the early seventeenth century. Developments were piecemeal and uncoordinated, enjoying only fitful royal backing. Here we are far removed from the world, portrayed for an earlier generation by G. L. Beer, in which, "the crown pursued an expansionist policy based on a set of assumptions and intentions later to be called 'mercantilist'" (p. 13). Elizabeth I's aims are seen, rightly,

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as being essentially defensive, with a low priority being given to oceanic enterprise. Certainly, no blueprint for action existed, and both Elizabeth and James I were reluctant to give active support to schemes that seemed unlikely to yield dividends, either in terms of money or power. Nevertheless, progress was made. Toward the end of Elizabeth's reign London merchants took more interest in distant waters, and, once the war of plunder ended in 1603, they had spare funds to devote to new projects of colonization and exploration.

By 1630 small but significant advances had been made: after much difficulty the Virginia colony was established; the English had various toeholds in the Caribbean and in New England; trade with the East Indies had been established despite continuing Dutch hostility. With hindsight we can see that these roots of empire were to bring rich rewards, but in 1630 the yield from 150 years of effort, involving an enormous expenditure of men and capital, might have seemed poor. This fascinating story has never been told better. It is beautifully written, and the volume is visually most attractive. Andrews's book is compulsory reading for all students of early modern England and for all with more general interests in maritime affairs.

DONALD WOODWARD
University of Hull

JASPER RIDLEY. *Henry VIII: The Politics of Tyranny.* New York: Viking. 1985. Pp. 473. $24.95.

Jasper Ridley's book is a disturbing case study in what a trained historical interrogator can do to the so-called facts of history: he can force them to say almost anything he desires. Ridley's purpose is to demonstrate that Henry was not only a first-class cad and consummate dissembler from start to finish but also a calculating, greedy, self-centered, cruel, albeit cautious, Machiavellian as well as an expert public relations artist who—J. J. Scarisbrick and G. R. Elton to the contrary—called every shot throughout his long and stormy reign and was a past master at getting others to do his "dirty work" and avoiding the blame.

Ridley uses every trick of the polemicist's trade. Almost from the moment Henry is introduced, reference is made to his "small, piggy eyes" (p. 39). Twenty-four pages later they are again used to effect: "the cruel piggy eyes of this tall, golden-haired hero" (p. 63). And, lest the reader should forget, the concluding words of the biography are, "his cruel, piggy eyes" (p. 416). Ridley is also a master of guilt by association. Henry's government is said to be "remarkably similar to the totalitarian régimes of the twentieth century" (p. 18), and the king, "like all dictators, had an extraordinary fear of even the smallest and weakest group of refugees who were working against his régime from the safety of foreign countries" (pp. 88–89). Raphael Holinshed's famous, or infamous, estimation that seventy-two thousand thieves and vagabonds were executed during Henry's thirty-eight-year reign is, in the name of statistical accuracy, reduced to sixty-thousand and then translated into a percentage of the population—2 percent of 2.8 million—so that it can be compared to Hitler's extermination of 6 million Jews, also said to be 2 percent of the population of occupied Europe, and Stalin's death toll of 5 percent of the Russian population, or 10 million people (pp. 281–82). Finally, when Henry himself died, the reader is told, probably correctly, that "his counsellors were stunned and grief-striken by his death" and is then presented with the gratuitous information, "as were members of the Politburo when Stalin died" (p. 413).

Character assassination is also achieved by the skillful juxtaposition of contrasts. Whenever Henry was in any way responsible for some particularly nasty event, Ridley heightens its repulsiveness by describing the scene and then picturing Henry as disgustingly cheerful. When John Frith died in slow agony at the stake, "Henry was in happy mood when he left on his annual progress" (p. 224). After Bishop John Fisher is beheaded on Tower Hill, Henry is said to be "in excellent spirits" (p. 247). The same technique is used when, after the Pilgrimage of Grace, Lady Bulmer was burned ("The King was very merry in the summer of 1537" [p. 296]), when the Scots burned Lady Glamis ("Henry was merry" [p. 300]), and when the Protestant Robert Barnes and the Catholic Thomas Abel were executed in an ecumenical holocaust (Henry was once again "merry" and very much in love with his newest wife [p. 346]). Such a monster cannot be afforded the slightest benefit of the doubt, and Ridley dismisses the belief that Henry was genuinely distressed at the death of his third wife, Jane Seymour, as an unwarranted assumption based "on the fact he neither divorced nor beheaded her" (p. 303). Like an angel of vengeance, the author grinds and tortures the facts to reach his final conclusion. "Apart from his weapons of terror and propaganda, Henry won the support of the majority of his people by appealing to their worst instincts—to their hatred of Scots and Frenchmen, of idealists, martyrs, and 'do-gooders,' to their willingness to denounce their neighbours to the authorities" (p. 416).

The ultimate irony, of course, is that Ridley's argument—that Henry was indeed his own master, not only reigning but also ruling and that he was responsible for the decision and most of the means leading to the break with Rome—is perfectly tenable and possibly even correct. Unfortunately, the thesis is so grotesquely overstated, the rationality imposed

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on the king's every action is so rigid and devoid of any room for chance, mishap, or mistake, and the cultural climate of the century and the mind-set of the king so ill understood that the final result, which incidentally displays most of the failings of undergraduate expository writing, must be regarded as yet another dangerously anachronistic piece of journalistic polemic only faintly veiled in the fig leaf of scholarship. That Ridley should have indulged in such polemics is both a pity and a mystification, for he is a noted historian who has written two perfectly respectable biographies of important sixteenth-century figures: Archbishop Thomas Cranmer and Bishop Nicholas Ridley.

LACEY BALDWIN SMITH
Northwestern University

G. W. BERNARD. *The Power of the Early Tudor Nobility: A Study of the Fourth and Fifth Earls of Shrewsbury.* Totowa, N.J.: Barnes and Noble or Harvester, Brighton, England. 1985. Pp. xi, 228. $27.50.

This book examines the political, military, and economic activities of an important noble family from 1513 to 1560. It is less a study of the early Tudor nobility than a collection of interrelated essays analyzing the fortunes of the Talbot earls of Shrewsbury. Great emphasis is placed on the loyalty of the fourth earl during the Pilgrimage of Grace and his efforts to prevent the Yorkshire rebels from advancing beyond Doncaster. In 1549 the fifth earl supported the deposition of Lord Protector Somerset and managed to reconcile himself to the leadership of Northumberland. Shrewsbury initially accepted Edward VI's alteration of the succession but changed sides when Mary's superior strength became apparent. It was through military service, G. W. Bernard argues, that the nobility made itself indispensable to the crown. The earls of Shrewsbury successfully maintained law and order on their estates and served in France and Scotland. The Talbot estates, the most important of which were in the vicinity of Sheffield, grew in size between 1537 and 1553 but stagnated in value. Nonetheless, the earls maintained their regional influence and avoided serious disputes with their tenants.

In an important concluding chapter Bernard argues persuasively for the influence and power of noble families like the Talbots. In his view there was no early Tudor crisis of the aristocracy. He follows K. B. McFarlane in contending that the king's greatest subjects were councilors, not aspirants for offices involving administrative duties. Bernard believes further research on the early Tudor nobility should concentrate on careful studies of individual families and avoid dubious statistical analyses and grand explanations that extend beyond the sources.

This short volume, offering sensible insights and sound conclusions, connects the early Tudor nobility more closely with its medieval predecessors than with the Elizabethan and Jacobean nobles who have been the subject of more extensive research and controversy. Bernard's chapter on the Pilgrimage of Grace benefits from the influence of his mentor, C. S. L. Davies, and the sections on the reign of Edward VI wisely reject the myth of Northumberland as the wicked duke, although the author fails to recognize the extent to which Northumberland shared power with other councilors after 1549. The book is based on substantial archival research but neglects the Thynne manuscripts at Longleat and the letters of William, Lord Paget; moreover, the author's handling of his evidence—especially of the Yelverton manuscripts, the narrative of Antonio de Guaras, and ambassadorial reports—is not always sufficiently critical. Bernard also has an annoying habit of citing unpublished seminar papers in the footnotes, a practice that is a disservice to the reader as well as the reviewer. Because of a shortage of documentation, the earls of Shrewbury often get lost in lengthy discussions of issues that are secondary to the main subject. The Talbots were important during the early Tudor period, but the author really makes a better case for the significance of the nobility as a whole than for the particular contributions of the earls of Shrewsbury, who were never leaders of the first rank. The conclusion, while weaker on the newer noble families such as the Seymours, Dudleys, and Pagets than on the older families, is a valuable corrective to studies that generalize on the Tudor nobility using only Elizabethan examples. In spite of its shortcomings, the book is a useful contribution that should be consulted by all specialists on the Tudor period.

BARRETT L. BEER
Kent State University

MICHAEL MENDLE. *Dangerous Positions: Mixed Government, the Estates of the Realm, and the Making of the* Answer to the XIX Propositions. University: University of Alabama Press. 1985. Pp. x, 256. $28.50.

In 1965 and again in 1981 Corinne Comstock Weston reminded historians of the importance of Charles I's *Answer to the Nineteen Propositions*. It gave royal sanction to the theory of mixed government, of government by king, Lords, and Commons. With Weston's emphasis on the important consequences of Charles's *Answer*, Michael Mendle has no argument, but he finds her account of the origins of the *Answer* defective. His book is an endeavor to give a less defective account.

He begins his study with a description of the reception of the classical theory of mixed govern-

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ment in Tudor England but insists that this theory was not intrinsically radical. The extrinsic factor that made it radical was Presbyterianism, which, by attacking bishops, attacked the three estates of clergy, nobility, and commons, thereby opening the way to republicanism and parliamentary supremacy. This transformation of the theory of mixed government occurred in Scotland, but in the 1580s Scotsmen brought it to England. It thrived there for some years, but from 1606 to 1640 the idea of the three estates as king, Lords, and Commons and of mixed government by these estates nearly disappeared from England. This occurred in part because of the decline of Presbyterianism, in part because Archbishop Bancroft branded the idea a "dangerous position" but principally because James I vigorously opposed the idea. In 1640, however, the idea returned by way of Scotland, causing the opposition to associate the revision of the estates with the cause of mixed government. Lord Saye and Henry Parker took the lead in this, but by the time Charles embraced the idea in the *Answer* they had deserted it. This great reversal came about because the opposition became frightened by the popular tumults against bishops and saw the need to fall back on ordinances passed by two estates only. Meanwhile, Lord Falkland and Sir John Culpeper, who wrote the *Answer*, turned to a revision of the estates and mixed government because they saw that it fit better with Charles's agreement in February 1642 to exclude bishops from the House of Lords.

Although Mendle writes with verve, elegance, and great learning, I find his argument unconvincing. Firstly, whether bishops were an estate is peripheral to the *Answer*. Charles had already agreed to the removal of the bishops from the House of Lords. Furthermore, the bishops, possessing no right of veto, long bereft of their allies in Parliament (the proctors and abbots), and sitting by right of their baronies, were not a clerical estate. Nor was their presence in the Lords an impediment to the attainment of a mixed government, as events after 1660 demonstrated: the bishops returned and mixed government emerged. Secondly, the true key to the *Answer* lies in the Declaration of May 26, 1642, which denied the king a veto, and the nineteen propositions, which denied him executive power. Seeing that Parliament would deny the king even a coordinate power, Falkland and Culpeper, as good debaters do, borrowed their opponent's rhetoric to oppose their cause. Thirdly, the connection Mendle draws between Scottish and English proceedings is tenuous in the extreme. He provides far too little evidence and offers far too much conjecture. In short, for an explanation of the *Answer* one would do better to read the works of Corinne Comstock Weston.

CLAYTON ROBERTS
Ohio State University

JOHN CANNON. *Aristocratic Century: The Peerage of Eighteenth-Century England.* (Wiles Lectures, Queen's University of Belfast.) New York: Cambridge University Press. 1984. Pp. x, 193. $34.50.

This is an expanded version of the Wiles Lectures given by John Cannon at Queen's University of Belfast in 1982. It has chapters on recruitment to the peerage; the education, religion, and marriage patterns of the nobility; and the political, economic, and ideological bases of aristocratic power throughout the eighteenth century. Cannon uses what he calls structural analysis and attempts to place English historiography of the eighteenth century in a European context. His book, he says, is "broad in character and of a pioneering nature" (p. vii). It contains a great amount of interesting empirical data; Cannon has done a lot of counting. His main argument is that the aristocracy was a closed rather than an open elite. In the eighteenth century this elite extended its political, economic, and ideological hold on power so that its dominance in these areas was the distinctive condition for England's successful industrialization. There was neither a "necessary" bourgeois revolution nor a significant bourgeois challenge to aristocratic power, and no challenge at all from the rest of society. There was only, or overwhelmingly, accommodation and deference to aristocracy.

In the argument Cannon criticizes Harold Perkin's emphasis on the openness of the aristocracy and, using class to mean social stratification, takes issue with his notion of eighteenth-century England as a classless society. (In passing, he also takes the historians' mandatory swipe at the inadequacies of sociology.) Cannon also challenges economic historians to come up with better explanations for the prosperity of the peerage in the period. He offers his own "noneconomic" explanations, as if reality is best constructed in terms of the antinomies of the academic division of labor in the twentieth century. And he attacks so-called economic interpretations of reality. Indeed, the book begins and ends with this concern.

The main target of Cannon's empirical, structural analysis (I can find no theory in it, only montage) is the Marxist interpretation of history. This has "caused Marxists to postulate a vast chronological misplacement, anticipating developments by two hundred years or so, and ignoring the nature of the eighteenth-century political regime" (p. 177). Cannon seems to suggest that class analysis has no place in eighteenth-century English history. Yet he discusses neither the theoretical, structural, conflictual, or hegemonic arguments in the materialist conception of history nor class analysis and only refers to Christopher Hill's writings on the revolution of 1641.

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For my part, I find little difficulty in interpreting Cannon's counting within a theoretical, materialist conception of history with the concept mode of production at its center. Accordingly, although Cannon may believe he has delivered a death blow to the ideas of Christopher Hill, no one should suppose that anything Cannon has written has any bearing on the materialist conception of history. Marxist historians are not all to be tarred with Cannon's theoretically sloppy brush.

R. S. NEALE
University of New England

ALAN J. GUY. *Oeconomy and Discipline: Officership and Administration in the British Army, 1714–63*. Dover, N.H.: Manchester University Press. 1985. Pp. x, 188. $37.50.

Alan J. Guy makes two contributions to the literature on early Georgian England. First, on the basis of his thorough research in printed and manuscript sources in the British Library, the Public Record Office, and other collections, he outlines the Hanoverian army's regimental organization, administration, and pay procedures. The brief text, two figures, twelve tables, and three appendixes make a useful reference book. The inclusion of a table showing the changing prices of commissions between 1714 and 1763, however, would have been most welcome. Second, Guy develops the observation made by Lieutenant-General Richard, Viscount Moleworth, in 1744 that "oeconomy and discipline" were quintessential marks of a good army. The author assumes that everyone understands that the general, a veteran of Blenheim and Ramillies, spoke with authority.

Discipline meant the "modern military virtues of uniformity and subordination" (p. 2). At issue is not the necessary, often brutal floggings that the British officers inflicted on the rank and file but the crown's historic effort to curb insubordinate officers. The Georges had little success, for, as Guy shows briefly, the aristocratic officers "were often deeply insubordinate" to the crown (witness the dust jacket's satirical cartoon of the duke of Cumberland) and to each other (pp. 37–42). Guy thus confirms the point that William B. Willcox made years ago in *Portrait of a General* (1964), where he documented how the friction between Sir Henry Clinton and Earl Cornwallis had disastrous consequences in the war against the American colonies.

As for Molesworth's other point, "oeconomy," Guy sets aside interests in frugality and concentrates on "clear-cut administration" with particular attention to the purchase of commissions. Historically, they were good investments because the perquisites of rank described in chapters 4 and 6 yielded substantial profits. The crown, however, saw the trafficking in commissions as a diminition of royal power, a source of corruption, and part of an irrational system of regimental finance. Thus, the crown undertook reform. The point is hardly a new one, thanks to Anthony Bruce's excellent monograph, *The Purchase System in the British Army, 1660–1871* (1980). Guy goes beyond Bruce, however, in arguing that the crown's efforts to nibble away at corruption by a disconnected, ad hoc succession of regulations, orders, and statutes had three major results. First, Guy believes that the crown's initiative raised the curtain on the economical reform that developed later in the century. Second, the reforms, by abolishing the perquisites of the junior officers, created major hardships for those who did not have substantial independent incomes. Many officers complained about their financial difficulties, as Guy observes, but few appreciated that the well was dry. Thus, young men continued to purchase commissions with the expectations of substantial profits—a point nicely documented a decade ago by Marion Balderston and David Styrett in *The Lost War: Letters from British Officers during the American Revolution* (1975). Third, Guy suggests an important two-sided development. On the one hand, the independently wealthy men continued to purchase commissions under a more closely regulated system and, curiously, developed a sense of duty and service previously unknown in the British army. On the other hand, the officers who needed to earn a living sought careers in India. This challenging thesis, the demonstration of which is outside the scope of Guy's study, deserves attention by future researchers.

WM. KENT HACKMANN
University of Idaho

MYNA TRUSTRAM. *Women of the Regiment: Marriage and the Victorian Army*. New York: Cambridge University Press. 1984. Pp. ix, 262. $39.50.

Myna Trustram analyzes the conflicts that existed between the military career and commitment of the English Victorian soldier and his family and sexual life. She demonstrates that antagonisms were always present between the purposes, functions, organization, and mobility of the army and the essentials of conventional domesticity. Each regiment administered army regulations with its own style of command and set of rules, and the regiments successfully competed with wives and children for the primary loyalty and obligation of the ranks. The army's goal, says Trustram, was to furnish the men with the personal services, attention to needs, and material environment that were associated with wives in civilian society. The military even attempted

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to provide an alternative to sexual life in marriage, first by encouraging the ideal of celibacy and later, in line with emergent theories of masculine sexual drives, by providing access to allegedly disease-free prostitutes through the legislation of the Contagious Diseases Acts of 1864, 1866, and 1869. Further discouragement of marriage is evident in the special status given to largely working-class soldiers that allowed them to escape from the civilian world of economic precariousness and the obligation to maintain dependent wives and children. The army was blatant in its preference for unmarried recruits and was successful in keeping the military marriage rate low from the 1850s to the 1880s, the period Trustram studies.

Those marriages that regiments did approve were evaluated by the commanding officers and then regulated so as to bring wives and children into the fold as "service families" that provided for the army's practical needs; wives, for example, were a source of cheap and easily controlled labor. Of course, the army, and perhaps the poorer families, might have regarded such employment as a benefit, just as army-governed schooling was seen as an asset for soldiers' children. Despite the security that employment and schooling provided, however, the expectation remained that the interest of the regiment came before family. Military discipline and regulation extended into the domestic circle. In dealing with these officially approved marriages—known as "on the strength"—Trustram evaluates and employs military sources effectively. She treats equally well the army's comparative callousness toward the marriage of soldiers that the regiment refused to approve, marriages known as "off the strength." In ten chapters arranged topically, Trustram explores questions of relevance: the relationship between army, state, and philanthropic welfare for all women related to the military; policies the army set for unmarried soldiers that winked at the Victorian moral code; concepts of male sexuality related to beliefs about social class and gender roles; the impact of the crusades against the Contagious Diseases Acts followed by the later campaigns for "social purity"; and the influence of developing Victorian domestic ideology and styles of civilian female philanthropy on the regimental assigment of authority and responsibility to middle-class officers' wives who were closely identified with the rank and duties of their spouses.

This is a welcome contribution to family history, social history, and women's history. The last would have been even better served if Trustram had focused as analytically on the women as she did on the military and if she had integrated her feminist perspective into the interpretive schema rather than imposed it on the discussion. As it stands, she sacrifices her opportunity to explore new insights and apply more innovative feminist scholarship in favor of reciting now-familiar themes that bear the marks of a "feminist canon." The book is nevertheless important and suggestive for further research.

BARBARA PENNY KANNER
University of California, Los Angeles

FRANÇOIS CROUZET. *The First Industrialists: The Problem of Origins*. New York: Cambridge University Press. 1985. Pp. ix, 229. $37.50.

François Crouzet is concerned with the social origins of the industrialists who pioneered the development of a new system of production. He defines this in terms of the occupation of their fathers and their own occupations before they became industrialists. An important initial problem is the definition of "industrialist," characterized here as one who owns a significant part of the capital of a large firm and is actively involved in its management, thus ruling out both sleeping partners and salaried managers.

This is not, as the author makes clear, a work based on primary sources nor one that pretends to scientific or random samples but, rather, an attempt to create from existing studies a larger, more representative "national sample" than has been used before. On the basis of this sample, the author concludes that the upper classes and gentry produced only a few industrialists and that the professions were not well represented, although, among them, lawyers made the most significant contribution. At the other end of the social scale were some success stories and a few dramatic rags-to-riches tales, but the working class contributed relatively few captains of industry. So it was the "business middle class" that swelled the ranks of the new industrialists, although the top layers of that middle-class group, connected with established elites, contributed only a small number, as did the merchant princes. Most first-generation industrialists were drawn from families already engaged in industry and, most commonly, in industries closely connected to the branch in which they would become factory masters. Large pottery factories established by men like Josiah Wedgewood, whose families had been handicraft potters for generations, provide a case in point. If industrialists with a working-class background were added to the lower middle class to create a "low born" group, that group would make up almost 50 percent of all first-generation industrialists.

Crouzet also tackles the myth of the self-made man, insisting that many modern scholars have defined the term too narrowly by applying it only to those from working-class backgrounds. In the nineteenth century a self-made man was considered to

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be one who was "the architect of his own fortunes," one who had built a fortune as opposed to inheriting it. There was no assumption that such a person would necessarily come from the lower classes. With this broader definition, "the much-derided and maligned lower middle class must be accorded a key role in British economic growth and the myth of the self-made man rises, phoenix-like, from its ashes" (p. 143). Crouzet's work is a fascinating study in its own right, and, because it draws extensively on the work of others, it also provides a good overview of the field. It makes an important contribution to the study of social mobility during the Industrial Revolution.

JANET ROEBUCK
University of New Mexico

TIM HILTON. *John Ruskin: The Early Years, 1819–1859*. New Haven: Yale University Press. 1985. Pp. xvi, 301.

A great many biographies of John Ruskin have been written. Some are worthless or practically worthless; others are in various ways admirable and useful. This first volume of Tim Hilton's study makes me think that his is the best. It has a verve and an exhilarating freshness that sets it apart from its predecessors. It reads as though the author had just discovered Ruskin and rushed into print enthusiastically to tell us of his discovery. This is, in a sense, the case, for Hilton wrote his book using a considerable quantity of new material, and, even where the biographer must perforce follow a familiar plan, he does so at a new pace and with a new purpose.

Previous biographers, seduced by the verbal magic of *Praeterita* and charmed by the polite hagiography of Wedderburn and Cook, those most obligating editors and biographers, have been unable to use, or have perhaps disregarded the work of modern scholars and the voluminous cornucopeia of Ruskin's still largely unpublished writings. In a generous foreword the author acknowledges the work of the latest generation of Ruskinians, starting with Helen Viljoen; clearly, however, he has done much research on his own account. Hilton's volume takes us to the point in Ruskin's life when he at last addressed himself to the task of finishing *Modern Painters* and broke with the strict evangelical faith of his mother. When the work is complete, it will, I think, give a new character to Ruskin studies—in itself a remarkable achievement.

Despite a few verbal oddities, this first volume is not simply a book for scholars; it is written with care and with good sense but also with a genuine enthusiasm for its subject. Hilton expresses himself with a vigor and often with a felicity that we hope for but do not often find in one who has kept such good literary company. Ruskin himself must be Hilton's chief concern, and he is treated with perceptive sympathy, as are Effie and Ruskin's parents, who in most biographies tend to be mere caricatures. Hilton also contrives, and it cannot have been easy, to deal effectively but perforce briefly with the large supporting cast that must appear in any life of Ruskin. He might have said more, however, about Ruskin's politics and psyche, but I imagine that these are topics to which he will return in his second volume, when he describes the Guild of St. George and Miss Rose La Touche.

Every serious scholar will have to acquire this volume, but even those who know practically nothing about Ruskin will find it enormously enjoyable.

QUENTIN BELL
University of Sussex

ROGER J. P. KAIN and HUGH C. PRINCE. *The Tithe Surveys of England and Wales*. (Cambridge Studies in Historical Geography, number 6.) New York: Cambridge University Press. 1985. Pp. xvi, 327. $49.50.

The most sustained and coherent methodology in British historical geography has been the cross-sectional approach established by H. C. Darby, with its special emphasis on the cartographic representation of large-scale, historical data sources like the Domesday Book. This definitive study of the tithe surveys for England and Wales by two former students of Darby is firmly in this tradition. According to Roger J. P. Kain and Hugh C. Prince, the tithe surveys are second only to the Domesday Book in their significance as a source of data, providing the basis for a wide-ranging view of mid-nineteenth-century agriculture. Their origin lies in the 1836 Tithe Commutation Act, which provided a defined basis by which the different forms of tithe in kind paid to the clergy, amounting to one-tenth of the total farm produce, could be commuted to a cash rent. In areas like the Midlands, some tithes had already been commuted and even abolished before 1836, but elsewhere they remained a source of grievance among the farming community, especially at a time when farmers were investing heavily in improvements only to see the local clergy reap a substantial part of the reward without any of the risk. The commutation act provided for a detailed survey of farm land, an apportionment of the tithe rent charge per farm or portion, and a file of the various reports and valuations generated by the work of the commissioners and surveyors.

As Kain and Prince make clear, the bulk and variability of tithe material make its systematic analysis or mapping a far from easy task. Their comprehensive review of past work also brings this point

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home, for, although a number of larger-scale studies have appeared as scholars realized the potential of the surveys, most studies have been at a purely local level. The authors review the problem of handling these data well and include valuable sections on the accuracy of the tithe maps and on the difficulties in standardizing and mapping information from the apportionments and files. The authors' advise that material on cropping and stocking should be treated as reliable only when used in aggregate form. Kain's own work on Kent has done much to establish procedures by which the tithe surveys can be used more easily to construct a regional and, ultimately, a national view of mid-nineteenth-century agriculture, establishing a computerized database so as to allow the speedier handling of material and using a SYMAP interpolation algorithm to enable a sample of data for select points to be converted into the form of maps. Examples of the regional maps produced by these techniques are provided in the text, but the full fruits are to be published in a separate atlas of tithe data.

Discussions of historical sources can be arid. Kain and Prince are conscious of this and have successfully enlivened their text by continually drawing out the usefulness of the tithe surveys to specific historical problems. This produces some satisfying discussions, but on some issues, like the relevance of the tithe surveys to the problem of field systems, they possibly allow themselves to be too drawn by the substantive problems involved, raising matters to which the tithe files can only make a limited contribution. Nevertheless, their stress on the tithe surveys as a valuable set of data, against which to measure earlier and later trends in, say, landholding, and as a source suitable for various types of record linkage, such as with the 1851 census, is well-founded. One wonders, however, how much more significant such a source would have been if the opposition to tithes had generated an earlier commutation act.

R. A. DODGSHON
University College of Wales

MARTIN J. S. RUDWICK. *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists*. (Science and Its Conceptual Foundations.) Chicago: University of Chicago Press. 1985. Pp. xxxiii, 494. $39.95.

The subject of this authoritative volume is a forgotten episode in the history of British stratigraphical geology of the second quarter of the nineteenth century, namely, the establishing of the Devonian system and period as internationally valid categories. The use in the book's title of the word "great" is justified on the grounds that the Devonian controversy, which began as a dispute about the identification and correct sequence of strata in Devonshire, was "great" to the scientists who participated in it.

The book is well structured. A relatively short first part sets the intellectual and institutional scenes; most of the participants in the controversy—including the principals Henry De la Beche, Roderick Murchison, and Adam Sedgwick—were prominent in the Geological Society of London. The long, narrative second part examines the controversy under a historical microscope. Martin J. S. Rudwick is doubtless correct in claiming that the episode can be "traced and analyzed with a richness of revealing detail that is probably unrivaled in *any* period of history" (p. xxi). Although the book is lucidly written, the detail of the second part will likely overwhelm all but the most avid historians of nineteenth-century geology. The author recognizes this possibility: "Readers who find themselves bogged down in the narrative can skip straight to the analysis in chapter fifteen, though they will miss much" (p. 13). This advice is unsettling, suggesting as it does that the heart of the book may be ignored, albeit at considerable cost. I seriously doubt that chapter 15 will make much sense to anyone who is unfamiliar with geology and its history and who has not read the second part. That chapter is one of two comprising the third and final part, which explores the implications of the case study for understanding the shaping, as the author prefers to put it, of scientific knowledge.

In his thorough and penetrating analysis of the controversy, Rudwick is several times critical of philosophers of science because they tend to be concerned with the logical structure of science as an abstract body of knowledge, whereas he stresses the day-to-day personal and intellectual struggles of practicing scientists. For instance, at one point Rudwick emphasizes that certain schemes followed developmental pathways and crossed interpretative boundaries "not in some disembodied Popperian world of ideas or theories, but in the lives and action of *persons*" (p. 409). Throughout his five stages of analysis the author makes further perceptive observations concerning the practice of science. One that applies to other disciplines, including history, reads: "For scientific status . . . could go down as well as up. The competence attributed to any individual could change markedly in the course of time, either upward through the production of work that others regarded as plausible and important, or downward by the production of work that was found implausible or trivial, or by simple failure to produce anything. Competence was not attributed once and for all, but earned and cultivated, worked for and maintained" (p. 420). With this book Rudwick has raised his already prominent status among histori-

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ans of geology in particular and of science in general.

The book has many useful diagrams, but it is annoying that references to them do not give the pages on which they are to be found. This is, however, a relatively minor complaint about an impressive book that I greatly admire.

WILLIAM MCGUCKEN
University of Akron

DOUGLAS CHARLES STANGE. *British Unitarians against American Slavery, 1833–65*. Cranbury, N.J.: Farleigh Dickinson University Press or Associated University Presses, London. 1984. Pp. 259. $29.50.

A host of monographs and scholarly articles documents the peculiar fascination that American slavery held for Britons in the wake of the Emancipation Act of 1833. Once slavery had been officially abolished within the British empire, evangelical Anglicans, Quakers, and Methodists remained at the forefront of abolitionist activity, redirecting their public addresses, sermons, pamphlets, petitions, and transatlantic correspondence. So did the British Unitarians, whose antislavery principles derived in part from their doctrine of universal brotherhood, in larger part from their identification with oppressed minorities. Considered "infidels" by orthodox churchmen, the Unitarians were the self-professed "theological negroes" of England.

At first glance, the Unitarians' antislavery crusade seems unworthy of monographic attention. Unitarians composed only 2 percent of Nonconformity; according to the census of 1851, they numbered merely thirty thousand active members in 229 places of worship. Yet Douglas Charles Stange insists that their influence far outweighed their inconsequential size because they provided a highly educated, articulate body of opinion from a financially successful and politically aggressive base. Unfortunately, the reader must take much of this assertion on faith, for the educational, occupational, and social status of the Unitarians is never submitted to scrutiny. Occasionally, a leading Unitarian spokesman is identified as a manufacturer or merchant, and at the very end of the book a passing nod is given to Howard Temperley's suggestion that economic interests worked in tandem with humanitarian zeal in the British antislavery movement. For the most part, however, this is a rather old-fashioned study of opinion that is largely divorced from the cultural context of mid-Victorian England as well as from other Unitarian concerns such as temperance and peace.

From an impressive mass of manuscripts, pamphlets, and newspapers, the author reconstructs Unitarian arguments that were admittedly articulate in some cases, abrasively earnest without fail. Against American slavery they argued most over means rather than ends. Like the larger body of antislavery opinion in Britain, Unitarians divided sharply between Garrisonian radicalism and more moderate proposals for reform. That the Garrisonians failed to win solid Unitarian support should come as no surprise in light of the supreme irony that many of the charities and chapels of England's most self-consciously liberal denomination were funded from a trust provided by the plantation profits of Robert Hibbert, a Jamaican slaveowner prior to 1833.

WILLIAM J. BAKER
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JANET OPPENHEIM. *The Other World: Spiritualism and Psychical Research in England, 1850–1914*. New York: Cambridge University Press. 1985. Pp. xii, 503. $44.50.

Perhaps because it is a product of the 1980s, this book is disappointingly timid. To a subject with exciting potential for the exploration of the late nineteenth century (as partly demonstrated by several scholars in recent years), Janet Oppenheim inflicts a relentless demureness. Although she fully succeeds in her aim to explore the Victorian "fascination" with spirits and psychic phenomena in terms of Victorian frameworks of understanding and evaluation, her accomplishment comes to little more than an "impartial" update of the more personally invested histories of the spiritualist movement by Frank Podmore (1902) and Arthur Conan Doyle (1924).

After an initial fifty pages that set the scene and social landscaping, the book divides more or less evenly into two parts. The first, which might have been denominated "Magic and the Decline of Religion," treats the various implications of spiritualism within the familiar acreage between science and religion and deals, in particular, with the agonzied victims of the proverbial "crisis of faith" in their search for a surrogate faith. For good or for ill, Oppenheim offers no simple explanation for the turning to spirits; although many of the persons she discusses appear to have become seriously involved in the spiritualist movement only after the deaths of persons close to them, their biographies illustrate, above all, the variety and complexity of responses to the phenomena in an age scientistically inclined yet struggling against the clutches of materialism.

After surveying (at greater length than would seem necessary) the so-called pseudoscientific and fringe alternative medical practices that fed into the spiritualist movement, Oppenheim settles down in the latter part of the book to writing what amounts to a series of full-length entries for a spiritualist

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dictionary of scientific biography. Central figures in the Society for Psychical Research who were also prominent in Victorian science are discussed according to their place in an implicitly positivist arrangement of soft to hard science, from Frederick Myers in psychology through Robert Chambers and A. R. Wallace in biology to William Crookes and Oliver Lodge in physics.

With its heavy reliance on biography, this book, not surprisingly, fails almost entirely to account for the spiritualist movement in terms beyond those of individual psychology. Evidently distrustful of sociological theorizing, Oppenheim fastens herself to the empirical sides of the deep, dark, ethereal well of spiritualism's history with hardly a glance at the well's rim, the larger historical totality in which the movement figured. Issues of class, gender, and politics are not entered into, except superifically; the origins and politics of the various spiritualist institutions are not discussed; and even individuals treated in detail are frequently seen to behave with "inevitability" (pp. 222, 233, 321). Content to go no further than to illustrate that spiritualism was an intellectual and religious response to materialism, Oppenheim feels a greater need to debate the veracity of certain recorded spiritual and psychic phenomena and to comment on the credulity of those who observed them than to ponder the social relativity of the truths disclosed.

This will satisfy few modern historians, least of all those seeking better to understand and historically to consider the changing and subtle relations between spirit and matter from the seventeenth century to the contemporary "turning point" depicted by Fritjof Capra. Nevertheless, we should be grateful for a richly documented, reliable, and fully accessible introduction to the nineteenth-century career and careerists of the other world.

ROGER COOTER
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MARVIN SWARTZ. *The Politics of British Foreign Policy in the Era of Disraeli and Gladstone*. New York: St. Martin's. 1985. Pp. xiii, 221. $27.50.

Marvin Swartz has tackled an important subject: the relationship of domestic and foreign politics in Victorian England during the ascendancy of Disraeli and Gladstone. His style is crisp and free from jargon, and his conclusions carry conviction because they are based on a wide and detailed knowledge of the sources. Reacting against Marxist (and Hobsonian) views of economic manipulation and the improbable spectacle of politicians becoming the puppets of the faceless men of finance, some Western historians have perhaps allowed the pendulum to swing too far in the opposite direction, insisting on the autonomy of foreign policy and the ignorance, or indifference, of politicians and high-ranking civil servants to economic considerations. Such simplicities rarely survive a thorough examination of the archives, for diplomatic, strategic, political, and economic motives intermeshed in a complicated way.

Swartz does not believe in any absolute primacy of foreign or of domestic policy, although he does believe that "for Lord Beaconsfield foreign policy was an extension of politics" (p. 82). The Parliamentary Reform Act of 1867 nearly doubled the British electorate to two million, or 40 percent of adult males. Politicians therefore had to reassess their relationship with the voting public. Canning and Palmerston had already shown how to mobilize opinion on foreign issues. Swartz perhaps exaggerates the contrast between politicians at the beginning and the end of the century. Castlereagh may have had the manner of a "cold and aloof aristocrat" (p. 1), but he came from an Ulster mercantile family, only recently ennobled. Disraeli believed himself to be of aristocratic descent and always identified with aristocratic interests. But Swartz is clearly right in contending that the milieu in which politicians then operated was undergoing fundamental changes, and on the whole British politicians survived the upheaval with remarkable skill.

At the same time, British policy makers had to "redefine their nation's commitments to its empire and to the European continent" (p. 5). The common denominator was the fate of the Ottoman empire, which involved both Britain's imperial communications and the balance of power in Europe. Swartz concentrates his analysis on the period from the eruption of the Eastern crisis in 1875 to the British occupation of Egypt in 1882. He examines in detail both the party conflicts created by the Bulgarian agitation and the stresses within the Conservative cabinet engendered by the whole Eastern Question. The latter came to center on the quarrel between Disraeli and Derby, and Swartz sees Derby's resignation in April 1878 as "a significant transformation of British politics." Until then, Disraeli needed Derby's prestige to protect him from the charge of being a "new man." After Derby's resignation he still needed to conciliate the landed classes, but "parties were becoming the predominant force in politics. Power lay with politicians who could command their allegiance and rally the masses in the country. Beaconsfield and Gladstone met the needs of this era of political transition, as Derby and Carnarvon, Granville and Hartington could not" (p. 85). The logical result was Gladstone's direct appeal to the electorate in the Midlothian campaign. Ironically, the Bulgarian horrors were not an issue of immediate concern to most of the electorate, nor is it likely that the campaign determined the outcome of the 1880 general election. Even more ironically, the

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Liberal government's first major foreign initiative, the occupation of Egypt, contravened all the principles of the Midlothian campaign, but the majority of the British public not only warmly approved of the occupation but also to some extent even forced it on the government. This is a book of many layers and interesting insights.

M. E. CHAMBERLAIN
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CORAL LANSBURY. *The Old Brown Dog: Women, Workers, and Vivisection in Edwardian England*. Madison: University of Wisconsin Press. 1985. Pp. xi, 212. $23.50.

In 1907 some minor riots took place when medical students from the University of London attacked the bronze statue of a brown terrier dog. The statue stood in a park in Battersea, the home of the last antivivisectionist hospital and the largest dogs' home in England. It commemorated a terrier vivisected in the laboratories of the university in 1903 and 252 other dogs killed there the previous year. The rioting medical students were countered by trade unionists (led by John Burns) and by suffragettes such as Charlotte Despard. The students subsequently attacked women's suffrage meetings, shouting "Down with the brown dog!" (p. 16).

This thoughtful book asks why the brown dog received the support of both workers and suffragettes, who cared little for each other and were not usually interested in the antivivisectionist cause. Coral Lansbury's thesis is that workers and suffragettes defended the dog not so much out of antivivisectionist beliefs but because the statue became a symbol of the victim, whether that victim was animal, woman, or worker. In developing this thesis she explores in detail the numerous parallels between the cruel treatment of animals in Victorian fiction and the treatment of women in Victorian pornography. She also analyzes the projection of human attributes onto animals in works such as *Black Beauty*. Her persuasive conclusion is that "the cause of animals was not helped when they were seen as surrogates for women, or workers, or when they were translated into fictions, no matter how appealing" (p. 188).

The strength of this book derives from the author's extensive exploration of the portrayal of animals and women in fiction. She is not, however, deeply versed in the history of the women's and workers' movements. On the book's first page she describes the vivisectionist rioters of 1907 as being opposed by "women who had shown they were ready to endure prison and force-feeding to secure the vote" (p. ix). Force-feeding was not introduced until the autumn of 1909. She then describes trade unionists as bitterly opposing women who broke windows and destroyed property, yet she fails to mention that the suffragettes did not do such things until 1908. In addition, her description of antipathy between the suffragettes and trade unionists is simplistic. The Pankhursts did not resign from the Independent Labour party (ILP) until early 1907, and during that year the causes of labor and of militant women were by no means as separate as they would later become. This book contains no mention of the successful campaign for Parliament waged by Victor Grayson during the summer of 1907, a campaign in which Grayson, an ILP candidate, embraced the women's suffrage cause and received active support from Emmeline Pankhurst, who spoke on his behalf. What Lansbury has not grasped is that the suffragette movement went through a rapid political transition between 1906 and 1910; in 1907 neither its political ties nor its tactics were what they would become a few years later.

Although its flaws are not insignificant, this book is wise, witty, and a pleasure to read. It is the work of a mature scholar who is not afraid to take methodological and intellectual risks. To the extent that it is a study of the ambiguity of symbols and the propensity of people to believe what they want to believe, the book is impressive.

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YOUSSEF CASSIS. *Les banquiers de la City a l'époque Edouardienne 1890–1914*. (Travaux de Droit, d'Economie, de Sciences Politiques, de Sociologie et d'Anthropologie, number 144.) Geneva: Librairie Droz. 1984. Pp. 449.

Those who read as they run or who read French slowly can be referred to a twenty-page summary of this book in the *Economic History Review* (2d ser. 39 [May 1985]: 210–29). The longer version is nonetheless heartily recommended. Youssef Cassis is little interested in economic as compared to social history, but he throws light on the organization of the City at its apogee, on the leading bankers and their relationships with each other, with the aristocracy, and with leading politicians. He is struck by the various sharp divisions among them. The top of the command pyramid consists of the merchant banks, which subdivide into different groups: the Quakers, the Anglicans, the old foreign, and the new foreign (many of whom were Jewish). The private deposit banks had been the equal of the merchant banks, but they had been gobbled up by the joint-stock banks, much larger than the merchant banks but below them in prestige and influence. The joint-

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stock banks, for example, were not privy to election to the Court of the Bank of England, which was manned by merchant bankers and merchants with a miniscule representation of shipowners.

The bankers were powerful and rich, descended from other bankers; none but a handful were self-made. Their power, however, was financial, not political, and, although they were elected to Parliament and formed pressure groups, they limited their exertions to banking and such economic questions as the budget, the gold standard, and free trade. On the last two of these endeavors, moreover, the ranks were divided.

The book proceeds from a sample of 360 bankers for whom Cassis has found detailed information from a wide variety of sources. Numerous tables deal with education, fortunes, marriages, residences, clubs, representation on boards of other firms (largely insurance companies, some shipping and railroads, but little industry). Pages are devoted to genealogies, especially of the Barclays, Samuels, Grenfells, Barings, and Smiths, and Cassis is astonished at the number of brothers who marry the sisters and daughters of other merchant bankers, members of the aristocracy, army and navy officers, and clergymen. (He also confesses to vertigo at contemplating the intermarriages of the Gurneys). Particular attention is paid to the profits of a sample of private and joint-stock banks (those for merchant banks outside of Rothschild were not available). The author notes that the profits were twice as high as those of the *Crédit lyonnais* in France. Of particular interest to me were the separation, socially and presumably in banking technique, of Manchester and Liverpool banks from those of London, and the intensity of the debate within the banking community between 1895 and 1898 between adherents of the gold standard and bimetallists (the arguments of the latter, however, are not given).

The book will give little comfort to the left wing. Cassis decides that political power and City money did not collude (p. 344). He also exonerates the bankers from the accusation that they were responsible for the war in 1914. This text has no more misprints than most modern books, but I cannot forebear calling attention to one possible bilingual Freudian slip: on page 74 Cassis refers to "mechant bankers."

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F. M. LEVENTHAL. *The Last Dissenter: H. N. Brailsford and His World.* New York: Clarendon Press of Oxford University Press. 1985. Pp. viii, 326. $34.50.

H. N. Brailsford (1873–1958) was probably the leading left-wing journalist in Britain between the wars, yet he is largely forgotten today. F. M. Leventhal, who has previously written on another forgotten man of the Left, the Victorian labor leader George Howell, has rescued Brailsford from undeserved neglect. Although Brailsford left little unpublished material, Leventhal has thoroughly combed archives and interviewed many of the journalist's friends and associates to produce a detailed account of his thoughts and his actions as well as moving glimpses of his private life.

Leventhal finds two themes of historical significance in Brailsford's career. One is his unusual position "at the crossroads of modern journalism" (p. 2), as a Victorian man of letters who became a twentieth-century mass journalist without surrendering his moral idealism. The other is his articulation, and stimulation, of the developing overseas concerns of British radicals and socialists through the first half of this century, from sympathy for the struggles of emergent nationalities and criticism of big-power rivalries leading to world war to agitation for international government and absorption in the Spanish Civil War and antifascism. Brailsford was the archetypal British leftist, not only in his ideas and involvements but also in his character: son of a Nonconformist minister, he transferred the intense moral commitment of that tradition to the stage of secular politics, becoming a scourge of the mighty and a friend of the oppressed throughout the world. He similarly carried on the Nonconformist devotion to conscience: scorning worldly advantage, again and again he refused to trim principles or compromise his integrity.

Leventhal's careful account is especially interesting on Brailsford's prewar criticisms of liberal foreign policy and on that forgotten classic *The War of Steel and Gold* (1914). This early attack on finance capital's supposed control of international relations proved a "formative influence," as Leventhal notes, on many labor activists. Brailsford's insistence, already put forth in 1909, that "the enemy is not so much the faulty reasoning of the many as the shrewd self-interest of the few" (p. 108) helped, for good or ill, sharpen the edge of public discourse on foreign policy. With Brailsford, as Leventhal shows, much more than with John Hobson or Norman Angell, we have left the ideological world of the nineteenth century for that of the twentieth.

This examination of an energetic and prolific career dedicated to the causes of peace and social justice is a major achievement, probably the most careful and thorough we have for any twentieth-century British left-wing intellectual save the Webbs (who of course left in Beatrice's diaries unparalleled material for the biographer). Leventhal, however, provides us with little sense of the texture of Brailsford's thinking and writing: Brailsford averaged about one hundred fifty articles a year from

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the 1910s through the 1930s, in addition to numerous books, but curiously few of his millions of published words are quoted. Nor are we given enough of a picture of how Brailsford appeared to other journalists and leftists to evaluate confidently his place in the history of his times. Leventhal frequently acknowledges Brailsford's lack of influence on events but does not consider why this should have been so. Perhaps this failure was not unrelated to Brailsford's outlook and style, which so enraged J. M. Keynes, who complained of his "carrying credulity to the point where it is almost certifiable" and of "bees in bonnets . . . without any balanced judgment or wisdom" (p. 270).

Nonetheless, Leventhal's book will be essential reading for students of the British Left in the twentieth century. By assiduously reconstructing a forgotten public life, he has restored a significant part of the turbulent history of our century.

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DAVID AYERST. *Garvin of the Observer*. Dover, N.H.: Croom Helm. 1985. Pp. 314. £25.00.

This review may suitably rest on a comparison of the James Garvin found in David Ayerst's biography with the one in Alfred M. Gollin's scholarly *The Observer and J. L. Garvin, 1908–1914* (1960) and that in the second volume of Stephen Koss's massive and often majestic *Rise and Fall of the Political Press in Britain* (1984). Gollin is perhaps the most objective in his examination of the years in which Garvin established his reputation in the British press, but he is as much concerned with *The Observer* as with its editor. Koss declares that Garvin, of all British journalists, best exemplifies the major themes of his study, but Koss is interested only in such detail as serves his purpose, and he is by no means always sympathetic. Ayerst seems to have few if any preconceived conclusions. His study provides the most detail of Garvin's life story—Garvin the individual, the family man, and the journalist—and is based to a considerable degree on family papers, many hitherto unused, along with the significant and comprehensive collection of Garvin papers at the University of Texas at Austin.

The comparison of these three historical works may be continued by examining their presentation of the developments that led to Garvin's appointment as editor of *The Observer*, London Sunday paper, in 1908. Gollin's detailed account is as much concerned with Lord Northcliffe, the proprietor, as with Garvin and is also much occupied with the general political setting. Koss, of necessity, provides little narrative and is generally interested in conclusions that he can suitably illustrate. Ayerst's story presents more personal aspects of both Northcliffe's and Garvin's lives, and he is also interested in Garvin's life outside the world of journalism. In his pages Garvin's appointment is no surprise.

The years of the budget controversy (1909–11) were almost revolutionary in atmosphere, and Garvin's editorship and his own column, week after week, established *The Observer* as the foremost unionist journal and Garvin's own role, in the words of John Grigg, as "almost equivalent to that of a parliamentary leader" (p. 86). Details, which Koss had to forego, are convincing in Gollin's story, but Ayerst's account makes such conclusions almost self-evident. We take in stride the conclusion that Garvin soon came to personify *The Observer*, for which he wrote from December 1907 until February 1942.

More than with most public figures, Garvin's entire life must be followed in order to understand fully his personality, character, achievements, and limitations. In Ayerst's pages Garvin reaches manhood virtually self-taught; our attention is arrested by his first newspaper writing, for the *Newcastle Daily Chronicle* at the age of 22, which "infuriated politicians but delighted readers" (p. 19). We appreciate as well as understand Ayerst's remark that Garvin became "a counter-revolutionary" (p. 87). He contributes greatly to our knowledge of Garvin's association with Northcliffe as well as with Waldorf Astor's purchase of *The Observer* in 1911. Koss's work is invaluable for following Garvin's role after the First World War; as a journalist he was the last of his breed because, increasingly, newspapers came to be dominated not by their editors but by their proprietors. But Ayerst spells this out and comes closer than either Gollin or Koss to understanding and appreciating Garvin. Through Ayerst, the reader will be able to do much the same.

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Amherst College

ALBERTO R. COLL. *The Wisdom of Statecraft: Sir Herbert Butterfield and the Philosophy of International Politics*. Durham, N.C.: Duke University Press. 1985. Pp. xvii, 173. $25.00.

We may be very grateful to Alberto R. Coll for giving us the first book on the thought of Sir Herbert Butterfield (1900–79). Coll reminds us of the extraordinary contribution made by that influential successor to Lord Acton, J. B. Bury, George Trevelyan, David Knowles, and others as Regius Professor of Modern History at Cambridge University. The book possesses the special merit of featuring a little-known side of Butterfield, his thought on international relations as found especially in his writings *Christianity, Diplomacy, and War* (1953) and *Diplomatic Investigations* (1966). Along the way we get

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looks at the works that established Butterfield's reputation as a historian: *The Whig Interpretation of History* (1931), *The Origins of Modern Science* (1949), *Christianity and History* (1949), *Man on His Past* (1954), and the monographs on Napoleon (1929) and George III (1949). We still need a thorough analysis and criticism of Butterfield as historian that fully uses all his published and ample unpublished writings.

Coll regards Butterfield as drawing on three distinct sources (history, politics, and Christianity) in an earnest quest for wisdom in international affairs and, accordingly, divides his approach to Butterfield into three sections (historian, political thinker, and Christian). The author, himself a political theorist, handles best Butterfield's political views on the balance of power, diplomacy, and the international system of states. He is noticeably less at home in the sections on history and Christianity, in which he offers more or less book-by-book surveys. He has only occasional success in keeping these two sections from pulling away from the book's theme of Butterfield on international relations.

On the whole this is a worthy presentation of Butterfield's thought on that theme: his affirmation of the merits of the balance of power, his realism about evil and power in world affairs, his concern for understanding all states' points of view in any conflict, his hope for pluralism, his warnings against self-righteousness, his dislike of American presumption, and much more. Coll expresses disagreement with only one position taken by Butterfield in his whole career: his support for unilateral nuclear disarmament.

There are significant misreadings of Butterfield, however. For one, the author treats Butterfield as a philosopher out to create a philosophy of international relations instead of as a historian writing essays to show how historical thinking might contribute to understanding international relations. For another, by compartmentalizing Butterfield's Christian perspective and treating it apart from his thought on history and politics, he misses much of the integrality of the thought as a whole. Most serious is his unbelievable interpretation of Butterfield as a conservative in the line of Richelieu, Metternich, Bismarck, Kissinger, and Nixon. Butterfield could as easily be interpreted as following from Marx. In his eagerness to speak to American conservatives during the Reagan years, Coll fails to recognize that Butterfield believed in noting the truth in almost any position while criticizing each one as well. He stood apart from the usual spectrum of views and interpreted the world from the viewpoint of one who held fast to Christ above all while being flexible on all the rest.

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GEORGE H. CASSAR. *The Tragedy of Sir John French*. Cranbury, N.J.: Associated University Presses, for the University of Delaware, Newark. 1985. Pp. 324. $35.00.

Neglected by recent scholars, Sir John French's career merits scholarly attention. This book is not a full-scale biography but a study that concentrates on French's reputation as a cavalry officer and his command of the British Expeditionary Force (BEF) in 1914. George H. Cassar confesses that, after surveying the evidence as objectively as possible, his opinion of French was less favorable than it had been before he began his research. Cassar's work is thorough, comprehensive within the limits of his objective, and very clear. Although the reader might wish for more information on French's early or personal life, these subjects are not a part of the author's declared purpose.

French entered the army in 1870 through the militia "back door" on his second try at the competitive examination for a commission. Because of his energy, ambition, and horsemanship, he soon caught the attention of his superiors. French participated in Wolseley's expedition to relieve General Gordon in 1884 and was "one of the handful of officers to emerge . . . with an enhanced military reputation" (p. 28). In 1889 he became commander of his regiment and later went to India with the Nineteenth Hussars. A period on half pay and various command and staff positions followed. French's military reputation, however, came largely from his performance in the South African War. While Cassar admits that French performed less effectively during the last third of his service in the Boer War than at the beginning, this was "the most successful chapter in his military career" (p. 60). French came out of the war convinced that quality of leadership was more imporant for cavalry performance than was technological change, and he set out to push this doctrine while holding the Aldershot command. Following Aldershot, French was promoted to field marshal and became chief of the Imperial General Staff. His resignation after the Curragh incident did not prevent the prime minister from promising that he would be recalled to command the expeditionary force in the event of war on the Continent.

French's record as commander of the BEF was disastrous. The author patiently traces French's inability to cooperate with his French allies, his minister of war, and his subordinate commanders. From the Mons retreat to the action at Loos, which resulted in French's recall, Cassar follows British operations in considerable detail. French regularly shared military secrets with his mistress but believed that Kitchener could not be trusted with the same information. On intelligence questions French

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"would not accept any reports that did not fit in exactly with his own views" (p. 162). A man of unstable temperament, French also "lacked moral courage, seeking to blame others for his own mistakes . . . [and was] obsessively jealous of his authority, vindictive towards some of his subordinates, resentful of his superior and incapable of working in harmony with his ally" (p. 292).

Although such snippets reflect the book's argument, they may mislead concerning the nature of the author's objectivity. The book is not a phillipic: Cassar's judgments are sound, his research is thorough, and his negative conclusions are justified.

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ALFRED GOLLIN. *No Longer an Island: Britain and the Wright Brothers, 1902–1909*. Stanford: Stanford University Press. 1984. Pp. x, 478. $35.00.

The bland treatment given by *The Daily Mail* in November 1906 to the recent achievements of Alberto Santos-Dumont, the pioneer aviator, led the newspaper's proprietor, Lord Northcliffe, to exclaim to a luckless subeditor, "Don't you realize, man, that England is no longer an island?" The Scots recipient of this reprimand may well have wondered if England ever had been an island. Nevertheless, the implication of Northcliffe's protest was a valid one. The invention and development of powered flight meant that, during the Edwardian era, British governments had to reckon with a threat to the security of their islands that the Royal Navy could do little or nothing to repel. Their response to this danger, the unimpressive record of the British military and naval authorities in designing aircraft of their own, and their dealings with Orville and Wilbur Wright constitute the main themes of this lengthy monograph.

The Wrights well understood the military potential of their airplanes, and Alfred Gollin considers in detail the difficulties the brothers encountered in trying to profit from their work. He also seeks to explain why, despite both the early interest taken by British aerial enthusiasts in their experiments and the fears aroused by Germany's rigid airships, neither the Admiralty nor the War Office was prepared to conclude a contract with the Wrights. The superintendent of the army's balloon factory was reluctant to recommend a course that might detract from his efforts and those of his subordinates to produce a successful aircraft. Even after the Wrights had publicly demonstrated the superiority of their machines, R. B. Haldane, the secretary of state for war, still declined to buy one. The British government preferred to experiment with a naval airship and to await further developments before acquiring airplanes.

This book is written in a style that will probably appeal to the general reader, and it contains information on the protracted bargaining between the Wright brothers and the British government that should interest air historians. But it has some serious shortcomings. The text is unduly repetitious, documents are cited at length when summaries would suffice, it contains no bibliography, and footnote references are sparse. Moreover, Gollin makes no attempt to analyze the consequences of Haldane's refusal to be stampeded by the Conservative opposition and the Northcliffe press into purchasing a Wright airplane. Would it have been more cost-effective for him to have acted otherwise? Was Britain any less well prepared in 1914 for war in the air than her Continental neighbors? The author's "remarkable conclusion" that "the Battle of Britain began when the Wright brothers flew" (pp. x–1) is also peculiarly facile. It would be just as portentous to say that the Norman Conquest commenced with the invention of the coracle or the Viking longship. Gollin has provided a fascinating survey of British reactions to the advent of aviation. As an essay on weapons procurement, it is, however, singularly inconclusive.

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KEITH JEFFERY. *The British Army and the Crisis of Empire, 1918–22*. Dover, N.H.: Manchester University Press. 1984. Pp. viii, 200. $35.00.

In 1918, at the end of a triumphant war, Britain faced vast new imperial commitments and new frontiers to garrison. The metropolis cried out for relief from its intolerable financial burdens, demanding both economy measures and a larger colonial contribution to imperial defense. But the colonies were in a rebellious mood, determined to resist any reassertion of closer imperial control or the assignment of their resources to broader imperial purposes. This picture will be familiar to any historian of the American Revolution.

Part of the compelling interest of this excellent book is that it will prompt such comparisons in the minds of its readers. This is all the more true since, as Keith Jeffery remarks, the political and military difficulties that confronted the British at the end of the First World War were in many respects a "dry run" for those that led to the disintegration of the empire soon after the end of the Second World War. Yet at first sight in 1918, as in 1763, the world had been made safe for the British empire. Britain's principal imperial rivals had been crushingly defeated (Russia by Germany). In a euphoric mood

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the British made plans to reinforce imperial security. Never again would a European conflict spill over and threaten the safety of the empire. Perfidious Turkey would be partitioned, Britain taking the lion's share. Iran would become a client state. Even Russia's old provinces in the Caucasus and Central Asia would become buffer states under British protection. The problems of imperial defense that had tormented prewar governments would be solved forever.

Deploying a wealth of archival resources, Jeffery describes crisply and clearly how this dream fell apart. For all these plans required not naval strength but army manpower. But, as soon as the war ended, two irresistible domestic pressures decimated Britain's swollen wartime army: the demand for demobilization and the insistent need for financial economy. The army shrank rapidly to less than its prewar size. The result was a desperate search for manpower. India was asked to maintain a larger army to meet the new needs, but the raj had its own troubles in 1919–22 and the viceroy dared not oblige. The dominions also refused. But what really turned the strategists' dream into a nightmare was the explosion of postwar turbulence all over the imperial system. In the Middle East the British encountered by 1920 a revival of Russian and Turkish power. In India, Egypt, and Iraq, nationalist unrest had to be contained. Worst of all, in Ireland the army found itself locked into a costly stalemate with the IRA. Even in Britain itself, large-scale industrial conflict frightened the government and strained the army's meager reserves.

In the 1920s these challenges to British imperial power did not prove as deadly as they first seemed. But, as Jeffery remarks, they were an indication of the limits on Britain's power and freedom of maneuver. For the student of Britain's imperial decline, this book will certainly be indispensable.

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ALAN BULLOCK. *Ernest Bevin: Foreign Secretary, 1945–1951*. New York: W. W. Norton. 1983. Pp. xvi, 896. $37.50.

This book should appeal to all serious students of the origins of the cold war since it offers a uniquely well-informed British perspective of international affairs between 1945 and 1951, when Britain still was one of the Big Three. Concluding Alan Bullock's three-volume biography of Ernest Bevin, it is, of course, primarily relevant to British politics, but it can be read on its own. Bullock uses a wide range of unpublished sources, notably, the newly opened Bevin papers and British cabinet records, as well as copious published British and American documents and secondary works. He writes fluently and successfully conveys the atmosphere and the mentality of the period.

Bevin's main concern was the rehabilitation of Britain and Western Europe through the involvement of the United States in Europe's long-term economic and military support. He was not deflected from his purpose either by constantly recurring crises and differences of opinion with the Americans or by the weakness of his position: Britain was economically exhausted, Western Europe was ruined, and, although Bevin commanded the loyal support of the prime minister and cabinet, he was for some two years exposed to strong leftist pressures in the Labour party to accommodate the Russians. Although coming from an extremely poor background and lacking formal education, Bevin was well equipped to cope with his gigantic task: not only was he endowed with truly exceptional intelligence and character but he also had very wide political experience. He had made a central contribution to the consolidation of the British trade union movement and to the mobilization of Britain's manpower during the war; he had, moreover, the background of considerable experience in international trade union and International Labour Organisation politics. As Bullock rightly stresses, Bevin combined a basic pragmatism with a sense of vision for the future of Western Europe and the world at large. Although beset by nearly continuous crises, often simultaneously in several areas, and, toward the end of his life, also by failing health, he did not lose his sense of direction and purpose.

A survey of Bevin's activities amounts to an account of immediate postwar history. For analytical purposes, Bullock concentrates on four closely interconnected areas: the Soviet and the communist threats, relations with the United States, rehabilitation of Western Europe, and the Middle East. Bevin was not involved in the administrative emancipation of India, which did not fall within his domain; he was enthusiastic and overly optimistic about the promise of the empire, transformed into the Commonwealth to buttress the power of Britain. He played, however, a central part in the decisions leading to the development of British nuclear weapons and was also actively involved in domestic economic planning and in the relations between the government and trade unions.

The analysis of Bevin's attitudes to the Soviet Union provides little support for the revisionist interpretation of the period. Bevin did not believe that the Russians intended to resort to war but was convinced that they would take the fullest possible advantage of any opportunities offered by Western weakness. Although temperamentally impetuous, he sat out the interminable meetings of the Council

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of Foreign Ministers as long as the slightest chance of agreement remained, but he was adamant in refusing unilateral concessions.

Perhaps the most revealing part of the narrative is Bullock's account of British relations with the United States, which were both intimate and complex. Bevin had to cope with an inexperienced president, an unsympathetic secretary of state (James Byrnes), a difficult Congress operating under isolationist pressures and lobbies (signally the one on Palestine). Britain was extremely weak economically and hopelessly overstretched in its worldwide commitments and was therefore completely dependent on the United States. Moreover, Bevin's position was complicated by the domestic susceptibilities of the British, the French, and, toward the end of the period, also the Germans, which were by no means clear to the Americans. It was to a large extent Bevin's personal achievement that he managed to arrive at a good working relationship with Byrnes's successor, George Marshall, and later a close friendship with Dean Acheson.

European rehabilitation and consolidation through the Marshall Plan and NATO and the establishment of the Federal Republic of Germany were largely a result of Bevin's initiatives. In all these questions he was ahead of public opinion, and he can scarcely be blamed for being unreceptive to the European visions of a federal union of Western Europe, an idea that appeared to the British not only as undesirable but also as completely unrealistic. Finally, Bullock carefully examines the controversial Palestine issue, using, among others, Zionist documents. He is persuasive in arguing that, contrary to widespread accusations, Bevin was not anti-Semitic. He was pursuing a policy opposed to Zionist aims only because these were irreconcilable with British obligations to the Palestinian Arabs. Because the Americans strongly supported the Zionists but adamantly refused to share the responsibility for Palestine, there was really no alternative to the inglorious withdrawal from the mandate.

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HARVEY J. KAYE. *The British Marxist Historians: An Introductory Analysis.* New York: Basil Blackwell or Polity, Cambridge, England. 1984. Pp. xii, 316. Cloth $24.95, paper $9.95.

Few would question the importance of the contribution to modern historiography made by the leading British Marxist historians. Rodney Hilton, Christopher Hill, Edward Thompson, and Eric Hobsbawm have each achieved a dominant position in their particular fields, and books like *Bondmen Made Free, The World Turned Upside Down, The Making of the English Working Class,* and *Labouring Men* have already established themselves as classics. More generally, these historians have exerted a significant influence on the changing concepts and methodologies of their discipline. The effects of Marxist historians on "history from below," class struggle analysis, cultural history, and the place of ideology and theory in historical studies are well known.

Harvey J. Kaye, of course, is not the first to attempt an assessment of the work of these historians. Raphael Samuel, Richard Johnson, and Gregor McLennon are among those who have already made decisive forays into this area of historiography. But Kaye's study is the most substantial to date; it is clearly structured and signposted and contains over fifty pages of endnotes packed with useful bibliographical information. Sandwiched between an introduction and a closing estimate of the Marxists' collective contribution are the five main chapters of the book, each devoted to an individual historian. Hilton, Hill, Thompson, and Hobsbawm, therefore, are treated fully; formative influences like Dona Torr and the Historians' Group of the Communist party are properly acknowledged. Maurice Dobb, to whom Kaye devotes a whole chapter, is, arguably, awarded disproportionate space since other leading Marxist historians such as Victor Kiernan, John Saville, and George Rudé are scarcely mentioned. Finally, the author includes too little on the Marxist historians' disciples and pupils.

Kaye's book, frankly, is hampered by both its structure and method. Separate, biographically defined chapters are not calculated to facilitate the making of effective comparisons; by and large, the reader has to work overtime to establish the patterns of similarities and differences. And Kaye's essentially descriptive method, offering potted summaries of almost everything these historians have written, can only be described as a lost opportunity.

What is clear is that, for these historians, Marxism has been not a strait jacket enforcing conformity but an incentive and challenge to explore, question, and disagree (even among themselves). Of the group, Hobsbawm is the only straight-line economic historian; Hill and Thompson are the only ones to make substantial use of literature. Thompson, the real oddball in every respect, is the only one of the group to be centrally involved in debates on the theory of history. Such distinctions become blurred in Kaye's treatment. Sadly, he provides only a description and not a proper assessment and analysis of the British Marxist historians. Too few questions are asked about the discontinuities and variations of the "tradition" they belong to, about their response to other outside stimuli (the Annales school, for instance), and about their effects on the historical profession

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in general and on the younger generation of historians in particular.

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RICHARD B. SHER. *Church and University in the Scottish Enlightenment: The Moderate Literati of Edinburgh.* Princeton: Princeton University Press. 1985. Pp. xix, 390. $47.50.

Students of the Scottish Enlightenment have for some time needed to steer a difficult course between specialist studies of those magisterial figures David Hume and Adam Smith and stimulating though often insubstantial essays on the character of the Enlightenment. A new generation of historians has recently begun to supply what is needed: solid, archivally based monographs on critical aspects of eighteenth-century society and culture in Scotland. Richard B. Sher's work is at the forefront of this development, and his new study, under the modest appearance of a collective biography of five leading Moderate clergymen, offers a major contribution to the history of the Scottish Enlightenment. Sher aims to place this group firmly within its contemporary political and social context and to reconstruct both its shared culture and its leading role in the "awakening" of polite literature. It is, therefore, an essay in that ambitious field, the social history of ideas.

The work is based on the careers of Hugh Blair, William Robertson, Adam Ferguson, Alexander Carlyle, and John Home, leaders of the Moderate group within the Church of Scotland who were equally significant as prominent figures of the Enlightenment. Much of the history of their rise to power is familiar, yet it has not previously been told with such authority and clarity. Many students will be grateful, for instance, for the way in which the complexities of the patronage disputes of the mid-eighteenth century are made comprehensible here. In the second part of the book the style and the doctrines of Moderate teaching are considered as a complex mixture of Presbyterianism, Christian Stoicism, and civic humanism, a blend that owed much to the continuing influence of Francis Hutcheson. Sher's stress on the influence of Stoic ideas is novel and welcome. The resulting ideology emphasized humanity, tolerance, and public virtue yet remained committed to the maintenance of existing social and political structures.

Sher points out other previously unnoticed facets of Moderatism—the unexpected use of the jeremiad as a preaching style, for instance. Moderate ideals, however, were most apparent in contemporary political debates. In the controversy over the Scottish militia, Moderate arguments were patriotic, looking for an infusion of public virtue. Moreover, these Moderates also supported and sustained the launching of the Ossianic poems, an episode that, Sher argues, can be understood only in light of their broader ideological concerns. But Moderate reaction to the American Revolution revealed fundamental antipathy to any political challenge to the existing order. Their ineffectual support for the principle of toleration, as embodied in the Catholic Relief Act, suggested their weakness and impending decline by 1779. Sher has some important comments also on the nature and cause of that decline. His presentation has problems, of course, associated with a limited number of subjects: at times, he needed to look more broadly at the extent of Moderate influence within the church, not only in terms of the management of the General Assembly but also at the cultural range and at the larger social influence of that score of liberal and enlightened clergymen, and others, whose work overall constituted this "clerical enlightenment" (p. 162).

Nevertheless, this is a most effective study, deeply rooted in previously underused archival sources. Scholars will be in Sher's debt, not least for his outstanding bibliographical essay, which reaches far beyond the specific concerns of this book.

JANE RENDALL
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LORNA JANE ABRAY. *The People's Reformation: Magistrates, Clergy, and Commons in Strasbourg, 1500–1598.* Ithaca: Cornell University Press. 1985. Pp. 272. $27.50.

American scholars such as Miriam Usher Chrisman and Thomas Brady have made significant contributions to the study of the Reformation in Strasbourg. Lorna Jane Abray's book on the "people's Reformation" is no exception. It traces the establishment of Lutheranism in Strasbourg from the 1520s to the 1590s. Although the author describes well-known developments in the first fifty pages, she covers new ground when she deals with the second half of the sixteenth century. It is the perspective from the 1520s to the 1590s that makes this book particularly interesting. Because German history in the second half of the sixteenth century has always been neglected, her study is most welcome indeed.

Abray describes the shifting character of the reformation settlement in the course of seventy years by pointing to the interplay of magisterial, clerical, and popular ideas of a Christian church. The reformation of the 1520s and 1530s, as an expression of the laity's distrust for the traditional clergy, was very much under the control of the magistrates, in spite of the pastors' drive to bring about a reform both of doctrine and morality. The anticlericalism of

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the common people and the magistrates prevented a resurgence of the old clerical domination during the following fifty years. This is why various small religious minorities were quietly tolerated in the city. From the 1570s on, the clergy began to win the full trust of the community. Threatened by a revival of Catholicism in the empire and mistrustful of alliances with the French Calvinists, the people closed their ranks in defense of the Lutheran church. But even then the magistrates reserved to themselves the right to make final decisions in church matters. Only toward the end of the sixteenth century, after a generation of leaders who had been brought up in the Lutheran church had taken office, did the magistrates finally accept the Formula of Concord and complete the Reformation by issuing the great new Church Ordinance in 1598. Although the clergy now imposed its clearly defined doctrinal position on the church, some aspects of their program such as the enforcement of strict moral codes were not accepted. Strasbourg retained what from its beginning had been a "people's Reformation."

This is the bare outline of a study that greatly enriches our knowledge of the particular form the Reformation took in this city. The author discusses a series of fascinating developments at Strasbourg such as the role and composition of the church assembly, the appearance and suppression of parish fellowships, and the changing role of the church wardens. The best chapters of the book describe the persistent defense of the magistrates against the subtle and not so subtle attempts of the clergy to increase their power in religious and moral matters. The marriage court and the morals court remained under secular control. No adult burgher was obliged to attend church in this city (really?). It is quite an achievement to describe, on the basis of archival research, the many forms that this interplay of clerical challenge and secular response took in the course of almost eighty years. The picture that finally emerges is that of a Lutheran church that was fully embraced by the overwhelming mass of the people of Strasbourg—a church in which the ideal of a saintly Christian city was not enforced but that still shaped the religious life and moral convictions of the population.

Abray has an enviable gift to present this complicated process in a lively and attractive form. Students will enjoy reading this book. But which book is without any flaws? The study is supposed to deal with the "people's Reformation," but the actors are almost invariably magistrates and leaders of the church. There is a chapter on the religious life of the common people, but the material for that theme is sparse. Abray mentions numerous leaders of the Strasbourg church but presents little information on them. Why did she not use the footnotes to tell us about their backgrounds and achievements? At times, these men appear like fleeting shadows.

I also wonder whether the religious settlement at Strasbourg was really so unique as the author suggests. Other free cities, such as Frankfurt, practiced even greater religious toleration than Strasbourg. The major free cities in south Germany maintained close contact with each other in political and religious matters. This dimension of the Strasbourg reform is not discussed. I also think that more justice could have been done to the dogmaticism of both the Strasbourg clergy and Protestant orthodoxy in general. Reading the chapter "The Triumph of Lutheran Orthodoxy," one almost gets the impression that these contentious discussions and convoluted volumes on true doctrine are really exaggerated. To be sure, Protestant orthodoxy is not easy to understand, but it represents an important intellectual effort and deserves more serious attention. Still, when everything is said, Abray's book is a truly original and valuable piece of scholarship.

CLAUS-PETER CLASEN
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STEVEN LAURENCE KAPLAN. *Provisioning Paris: Merchants and Millers in the Grain and Flour Trade during the Eighteenth Century*. Ithaca: Cornell University Press. 1984. Pp. 666. $39.95.

This important book, which is based on an exhaustive survey of the printed and archival sources, is well structured, elegantly composed, handsomely typeset, and carefully proofread. By its scope and scholarship, the book reminds one of a French *Thèse de doctorat*. Fortunately, however, the comparison stops there, because Kaplan's book is also a good read. Its implicit message on the quality of life, on the *mentalité* of eighteenth-century Parisians, is as convincing as its explicit exposition on the quality of the bread they ate. Steven Laurence Kaplan is not only wise; he also has a feel for his subject. This book is as subtle as it is learned.

Provisioning Paris will be, firstly, a source for those who are interested in the technological problem with which it deals: how Paris was fed in the last decades of the *ancien régime*. Readers will find here all that they wish to know about the rules of the baking game. How did grain find its way to Paris? When it got there, who sold it to whom and where? Were millers rich and oversexed? Did grain merchants make huge fortunes at the expense of the consumers? Were bakers endogamous or exogamous? What role did women and widows, especially, play in this business? How smoothly did the system run in times of dearth or drought? And how was grain ground and bread baked? Kaplan, who is *inter*

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alia a qualified baker, considers all of these positivistic questions with loving and fascinating care.

What interested me more, however, than this wealth of information—since historians do not live on archives alone—is the intellectual argument of the book, which relates to the social structure of the grain trade. In the main, the enterprise of provisioning Paris was a three-cornered affair that involved, first, the producers cum merchants; then, the public; and, finally, the agents of the state (or states one might say, since official jurisdictions often overlapped). These players functioned within two social modes—that of the market, where prices were set by the intersection of supply and demand, and that of the marketplace (that is, of regulated spaces), where the supply and price of grain was constrained by a wealth of rules whose justice seemed immanent to the players and whose enforcement the authorities saw as their appointed task. The strength of Kaplan's book, as I see it, is to show how those two principles applied differently at different times, along lines that were dictated by greed, common sense, and the wisdom of the ages. In times of abundance, the authorities became more slack. But they might also become more stringent, when the need arose, to the point of impinging on vested feudal interest. Although the trade did not attract altruists, and in spite of frequent conflict, calumny, insult, and litigation, there was little sense in any quarter that the provisioning of Paris—the sale of grain, bread, or flour—could be left to regulate itself. A corporate ethos of responsibility was everywhere present, even among the merchants who technically were not a corporate group at all. And, of course, Parisians were in the end more or less satisfactorily supplied. It will therefore come as no surprise that Kaplan is not particularly sympathetic to the growing number of liberal or physiocratic ideologues who from 1760 onward wished to change the system and liberalize the trade. His informed discussion of their sponsorship of scientific milling underscores their own uneasiness at what the practical consequences of their abstract policies might be.

The book as a whole points to an important conclusion—namely, that, although the institutions of corporatism had sadly declined in eighteenth-century France, the corporate spirit was still alive and well in that society. I was reminded here of some of Reddy's conclusions on the making of market culture and of Sewell's work on guilds. A body of literature, much of it of American origin, is growing in this field. We are now able to understand better than we did before the complex relationship in eighteenth- and nineteenth-century France of ideological discourse to reticent social reality. Kaplan's book is a significant contribution to the elucidation of that *problématique*.

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DAVID P. JORDAN. *The Revolutionary Career of Maximilien Robespierre*. New York: Free Press. 1985. Pp. xii, 308. $22.95.

In the fall of 1793 a remarkable document, the Instruction of the Temporary Commission to the authorities of the Rhône and the Loire departments, stated that, "to be truly Republican, every citizen must experience within himself a revolution equal to that which has changed the face of France." David P. Jordan suggests in this book that Maximilien Robespierre personally experienced such a spiritual revolution in 1788–89 and that this explains both his repeated expositions of principle and his exceptional hold on public opinion. Jordan thus attempts "a species of biography, intellectual biography" (p. 10). Using as his principal source Robespierre's own writings and speeches, Jordan seeks to show how Robespierre saw himself and the revolution from 1789 to 1794 and how he then sought to make his vision a reality.

So much has been written about Robespierre that the justification of a new book must lie largely in an author's ability to present his material, as Jordan does, from a fresh and stimulating standpoint. We have here Robespierre's perception of himself as the prophet of a great political struggle to effect a moral purpose—to transform the world by liberating the natural goodness of ordinary people. Examined in this light, Robespierre's political career is also apparently seen as an illustration of François Furet's argument that political power in the revolution went to those best able to express men's hopes and fears in the language of the hour.

The great difficulties of so considerable an undertaking affect Jordan's work. Being, like Robespierre, a lover of words, he has written a long book and one that includes much repetition. Moreover, to intermingle historical narrative with the more general consideration of ideology is dangerous, and Jordan yields too easily to the temptation to substantiate his analysis by transferring Robespierre's statements from one chronological context to another. More serious criticism must be made of how Jordan treats his subject's background, the revolution that really happened. Neither possibilities nor probabilities have any place in his picture; assertions frequently occur where cautious appraisal is imperative. For example, Jordan makes great assumptions about the intentions of Louis XVI in June 1791 and about the conduct of the Brissotins in June 1792. On occasion, such simplification produces startling errors of fact,

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like the statement that "the Brunswick Manifesto reached Paris on August 30" (p. 120) and so precipitated the prison massacres of September 1792. Unhistorical writing of this sort inevitably diminishes confidence in the author's general reliability.

This book nevertheless merits attention, for Jordan has something to say about Robespierre, and his summaries of Robespierre's position at particular times are often of interest. He succeeds, too, in impressing Robespierre's peculiar magnetism on the reader. Opinions may differ about whether Robespierre was as uncalculating as is suggested and about whether he should be so completely identified with all that was essential in the revolution. But Jordan's sympathetic and perceptive representation of him demands respect.

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GARY KATES. *The* Cercle Social, *the Girondins, and the French Revolution.* Princeton: Princeton University Press. 1985. Pp. xiv, 325. $40.00.

The *Cercle Social* has often been cited as a source of utopian socialist ideas, and Karl Marx himself called it the first true revolutionary group. Rather than tracing the lineage of the *Cercle Social*'s ideas into the nineteenth century, Gary Kates chooses to examine the social and political context in which the *cercle* operated during the French Revolution. The result is an original, lively, and richly documented study that traces the evolution of the *cercle* from its origins as a political coterie in the Paris municipal revolution of 1789 to its collapse after the arrest of the Girondin deputies.

The core of the *cercle* was made up of ten men, most of them from solid, provincial, middle-class families. The most notorious of them were Fauchet, Brissot, Roland, and Condorcet—all future Girondin leaders. They were brought together by a network of friendships based on common political and intellectual interests. Some of the friendships dated back to the 1770s, but the immediate occasion for the formation of the group was the struggle in Paris for control of city government. The Fauchetins had a stronghold in the Paris Communal Assembly of 1789–90, where they advocated representative democracy, not the direct democracy urged by the Paris districts nor the more authoritarian position claimed by Mayor Bailly. This characteristically intermediate political position proved problematic from the very beginning, and, despite the propaganda efforts of their newly established journal, the *Cercle Social*, the Fauchetins were soundly defeated in the municipal elections of August 1790. In response, the group withdrew from the political struggles of the moment and founded a new club, the *Confédération des Amis de la Vérité*, and a publishing house, the *Imprimerie du Cercle Social*, which published a succession of newspapers.

In 1790–91 the confederation devoted itself to the discussion of general philosophical issues and their application in the revolution. Of particular interest were their extensive discussions of civil religion, property rights, and the role of women. The confederation was the first club to admit women as regular members and the first to establish a separate women's section to demand equal rights for women. The club dissolved itself during the repression that followed the Champs de Mars massacre of July 1791 and did not reappear when martial law was lifted in August. By November 1791 the publishing house had become the mouthpiece for the revived Jacobin Club led by Condorcet and Brissot. Government subsidies were funneled through a sympathetic Paris city government until August 10, 1792, and afterward through Roland's newly created "office of public opinion." Girondin ascendancy was short-lived, however, and after the trial of the king the *cercle*'s publishing house ceased functioning as a propaganda center.

By carefully tracing the links between Girondin politics and the publishing history of the *Imprimerie du Cercle Social*, Kates is able to rectify many mistaken impressions about the *Cercle Social* as a group. The *cercle* was not a club of utopian mystics propounding unrealistic protosocialist ideas. According to Kates, the *cercle* manifested no socialist tendencies beyond a humanitarian concern for the laboring classes. The author convincingly demonstrates that the *cercle*'s publications consciously fostered the myth that the Girondins were a disciplined party opposed to Montagnard "anarchists," yet he does not portray the *cercle* as a party headquarters; the trial of the king demonstrated that the Girondins did not vote as a bloc in the Convention. Kates describes the *cercle* as "a Girondin agency designed to develop a political language to interpret the unprecedented events of the day" (p. 269).

The author could have pushed this analysis further. How was the *cercle* different from the Jacobins? A belief in "representative democracy" may have united the *cercle* adherents, but it did not distinguish them from their Montagnard opponents. The tension in "Girondin economic thought" between classical political economy and an emphasis on moralism was also present in Montagnard conceptions of economics. And what made women's rights and an early interest in redistribution of wealth so congenial to this group? Even though these important questions remain unanswered, Kates provides the reader with excellent material for rethinking some of the most critical issues of the French Revolution. He shows, moreover, that the history of the press can be

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of fundamental significance for the political and intellectual history of the revolutionary era.

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JOHN A. LYNN. *The Bayonets of the Republic: Motivation and Tactics in the Army of Revolutionary France, 1791–94.* Chicago: University of Illinois Press. 1984. Pp. xii, 356. $24.95.

This well-written volume, packed with information on the French army of the eighteenth century, is actually a detailed analysis of the Armée du Nord through 1794. In the tradition of Samuel Scott and Jean Bertaud, John A. Lynn has employed a quantitative and qualitative methodology to examine carefully the army's evolutionary development. This is not a study of campaigns, strategy, or operations but an examination of the life and motivations of the officers, whose inexperienced forces were transformed into an army that achieved the great victories of Wattignies, Tourcoing, and Fleurus.

Using documents at the Archives de la guerre and other primary materials, Lynn examines conscription, composition, motivation, training, equipment, and tactics in order to determine the strengths and weaknesses in each branch of the army. His application of sociological, psychological, demographic, and political science research tools produces a provocative and invaluable profile of the rank and file of the Armée du Nord.

In considering the role of military justice, administration, command, organization, political indoctrination and psychological impact on the rank and file, Lynn analyzes the wartime enthusiasm of the soldiers. Discipline is scrutinized in almost microscopic detail to determine the relationship between the egalitarian soldiers and their commanders. Careful distinction is made between the citizen-soldier's obedience to law and to his superiors. Motivational factors that set the French apart from other European soldiers are clearly examined along with the means employed by the revolutionary government to insure the loyalty of the citizens who fought to preserve their newly acquired rights.

The transformation of the officer corps is considered in the context of the revolution. Lynn points out discrepancies in assuming that most of the artillery officers of the *ancien régime* remained loyal to the revolution. Even more enlightening is information that traces the efforts of the government, ever fearful of the influence of the officers, to encourage the rank and file to suspect their commanders' motives. Lynn dispels other long-accepted assumptions by arguing that the French troops were not a highly emotional, undisciplined mob that gained victory by sheer numbers but a reasonably well-trained, effective, and flexible force led by officers who reflected similar socioeconomic backgrounds. Indeed, in every chapter various accepted views are made suspect or refuted by Lynn's evidence. He finds that the "cult of the bayonets" was unsubstantiated, that the government's exaggerated view of the fighting qualities of the *sans culottes* was without justification, and that élan was only one of a number of complex factors that motivated the men. I am less convinced than the author, however, that the soldiers, even if relatively successful in civilian life, "would almost certainly have excelled on the battlefield under any commander, fighting for any *acceptable cause*" (p. 170). Although many of the elements that spelled success for the Armée du Nord were present in the armies of the republic, Lynn is quick to admit that each of the other armies had its own idiosyncratic characteristics, reflecting differences in command, organization, tactics, and so forth.

This study is undoubtedly a major contribution to the literature of the French Revolution and an invaluable tool for those who seek to explain the military success of the revolutionary armies. The book's wealth of details, the many new and important observations derived from the methodology, and the valuable conclusions make it a model for those who seek to do for the other armies of the republic what Lynn has done so successfully for the Armée du Nord.

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PETER WETZLER. *War and Subsistence: The Sambre and Meuse Army in 1794.* (American University Studies, ninth series, History, number 9.) New York: Peter Lang. 1985. Pp. xxvi, 312. $35.00.

Peter Wetzler has written on a rather specialized aspect of military history, but one that influenced many areas of public policy. He is concerned with the efforts to subsist the revolutionary armies in Belgium and Germany, primarily during 1794, when the fortunes of war changed dramatically. In April of that year, allied armies, approximately equal to those of France, menaced the whole northern frontier and occupied several fortresses inside France from which they threatened an invasion. The outcome of the campaign was by no means clear at the time. Yet by June the French were completely successful. They had occupied Belgium, and in the fall they conquered most of the Rhineland and the Netherlands. The Army of the Sambre and Meuse was formed in late June 1794 and, by its victory at Fleurus, almost immediately opened Belgium to conquest.

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Wetzler is not concerned primarily with military operations and merely sketches them in as background to the problems of subsistence, which are his main interest. These were numerous and complicated, since various interests, all important to the war effort, competed for scarce resources. Great expectations were entertained for more provisions once the resources of Belgium were available. All authorities—military, supply service, French civilian, and local indigenous—agreed that supplying the armies was the first priority. Depleted in provisions by many months of warfare, however, the northern French departments hoped to draw quantities of supplies for their own relief. The Belgian peasants also had to be given enough for their support and for planting the following year. The occupied cities had to be provisioned. The various armies, particularly the Northern Army and that of Sambre and Meuse, competed for areas from which to draw their subsistence. The problems were complicated by the attempts of the French government to apply the policy of price controls to the occupied areas and by the question of occupation versus annexation.

Wetzler deals in detail with the rivalries these problems engendered, the influence of the policies of the changing central government on them, and the various local governing bodies set up to try to cope with them. Despite all these efforts, the subsistence of the Army of the Sambre and Meuse remained precarious and the troops suffered much hardship, especially in the winter of 1794–95.

Most works of military history do not give much space to subsistence problems, beyond dealing in general terms with their influence on operations. Wetzler has taken us into that area of military affairs, showing their influence not only on operations but also on policies of the central government, on the occupation, and on the conflicts among the occupied areas. His work is specialized but valuable to the student of the warfare of the period. His research is extensive and detailed. Some of the maps need more locations in order to follow the operations. There are numerous appendixes, including one on the social and professional backgrounds of the generals and supply commissaries that make up, in part, for the absence of an index.

ROBERT S. QUIMBY
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ALAIN PLESSIS. *Régents et gouverneurs de la Banque de France sous le Second Empire.* (Travaux d'Histoire Ethico-Politique, number 44.) Geneva: Droz. 1985. Pp. 444.

ALAIN PLESSIS. *La politique de la Banque de France de 1851 à 1870.* (Travaux d'Histoire Ethico-Politique, number 45.) Geneva: Droz. 1985. Pp. 354.

These two books are the second and third volumes of a three-volume work representing Alain Plessis's thesis at the University of Paris. I have not seen the first volume on the stockholders of the Bank of France during the Second Empire, but it is clear on the strength of these two that Plessis has established himself among the handful of distinguished financial historians of the nineteenth century in France.

The second of the three volumes covers not only the regents and censors but also the governors and deputy-governors of the period, examining on the basis of probate and notorial records these individuals' origins, connections, mind set, and, especially, their careers and wealth. In all, 800 people are covered, with detailed information on the wealth of 270 of these. The individuals are divided into three groups: the aristocrats, functionaries, and members of the liberal professions in the first, largely among the stockholders; the simple industrialists and merchants in the second; and leading bankers and industrialists in the third. The member of this last group dominated the Bank of France, although their decisions were by no means always unanimous. Differences arose owing to religion and, especially, to business interests. The leading banker, Alphonse Rothschild, for example, always wanted higher interest rates or at least delays in lowering them. Eugène Schneider, the steel magnate, favored lower rates to help business. As the author states, this volume is a social history of the economic elite of the Second Empire.

The third volume is old-fashioned, descriptive, and narrative economic history. Plessis occasionally digresses, as when he relates the discount rate in the period when it was altered, between 1857 and 1865, to changes in the bank's gold and silver reserves. The initial chapter, which describes the environment in which the bank worked and discusses the finance ministers of the period and their backgrounds, could as readily have been included in *Régents et gouverneurs*. Thereafter, the author takes up the major issues of the period chronologically, relying primarily on the often-bland minutes of the Council of Regents and on contemporary periodical literature, especially the well-informed and uncensored *Indépendance belge*. In so doing, he places a number of obscure topics in the history of the Bank of France in the limelight. First, he discusses the extensions of the bank's charter in 1852 and 1857, the extension of advances to railroad bonds (an unusual central-bank asset), and the close call with inconvertibility (*cours forcé*) in 1855. Then, Plessis describes the adoption and experience of discount-rate flexibility from June 1857 to July 1865, when there were forty-eight changes, after the maintenance of a 4 percent rate from 1803 to 1847 and from 1852 to 1855; regrettably, he does not provide an account of the reversion of the bank to a fixed

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discount rate. He considers as well the purchases of gold at premiums, the conversions of *rentes* by Jean-Martial Bineau in 1852 and Achille Fould in 1862, and the help given the Bank of France in 1860 and 1861, not only by the Bank of England after an initial mistake that led to a "war of central banks" but also by the State Bank of Russia and the Bank of Amsterdam.

Finally, Plessis shows the triumph of the bank and the Establishment over Emile and Isaac Pereires, Jules Mirès, and their adherents (including the economists) and discusses the background of the inquiry into the monetary policy of 1865–67, in which the Bank of France was vindicated. Plessis sets out the history in fascinating detail, exploring in depth the subtle differences among bankers, merchants, industrialists, aristocrats, and functionaries. It is sad that this superb, traditional economic history was not available earlier as background for the recent study by the National Bureau of Economic Research on the gold standard in the nineteenth and twentieth centuries, which unaccountably lacks an essay on France.

If it is not too churlish to complain when one has such a feast, one misses a bibliography, although that may be in the first of the three volumes. The author is wordy and he slights some technical aspects of interest to me, such as the mint ratio of gold to silver, which led to the undervaluation of silver in Paris. Plessis is also largely unfamiliar with the parallel literature on the Bank of England. He mentions the work of Richard S. Sayers and a study in French by Arthur Bloomfield but neglects, for example, the contributions of Frank W. Fetter on the development of British monetary orthodoxy. Likewise, he is unacquainted with the work on France in English by Rondo Cameron and David S. Landes.

Despite these blemishes, one must applaud a magisterial work that throws clear light on the evolution of the Bank of France, the economic history of the Second Empire, and the functioning of the gold standard. It belongs in every major library and should be read.

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RONALD CHALMERS HOOD III. *Royal Republicans: The French Naval Dynasties between the World Wars*. Baton Rouge: Louisiana State University Press. 1985. Pp. 221. $25.00.

It is striking that little attention has been given to the history of the French navy and its leaders during the interwar years. The army, owing in large part to its collapse and defeat as well as to the wartime collaboration of some of its leaders and the resistance of others, has obviously won attention, as have civil-military relations. From the army, Philippe Pétain, Maurice Gamelin, Maxime Weygand, and Charles de Gaulle were great actors in a national tragedy; from the navy, only Jean Darlan played a part, and his was quite modest by comparison. Yet the navy was favored by politicians of many persuasions, was supported by every parliament after 1919, and was significantly modernized during the interlude from the end of one war to the beginning of another. The navy's leaders were of course persuaded that it was central to national defense, and, like the generals, they were convinced that the government was not equal to the tasks demanded of and by the nation. Isolated socially and politically—by their own choice—naval elites drew further inward in the 1930s: some embraced a despondent extremism in politics, and many more were afflicted with defeatism. Given the clear parallels to the experience of the army as well as its intrinsic importance, the navy merits close study.

Ronald Chalmers Hood III's work is limited to the naval officer corps, specifically, to its social origins, recruitment and training, and military and political activities, especially during the 1930s. Hood offers a clear and convincing social profile of the navy's *grand corps*: by origin, inclination, and education, its members constituted a relatively closed, self-replicating society. Individual success was linked to "connections," family background, religious faith, and political conservatism. With respect to officers' conservatism, however, and on their political engagement in the turmoil of the 1930s, the study is disappointing. Hood finds that officers sought confrontation with the political process and that from 1932 "a single voice rose to dominate the naval officer corps, which in political terms expressed itself through the Action Française" (p. 125). He is also convinced that officers "remained anti-semitic in outlook" (p. 119). Doubtless many individuals were anti-Semitic and were diehard partisans of Charles Maurras. But Hood provides only clues, failing to explain convincingly why this may or should have been true for nearly the entire corps. And, given that the navy was so favored by politicians, even by those of the Popular Front, and was "better prepared" in 1939 than either the army of the air force, why should defeatism have been so thorough and the Action Française so appealing? Even allowing that Hood's interpretation may be close to the mark, his argument remains unconvincing, primarily because of his unyielding commitment, and audacious resort, to broad generalizations.

Despite its shortcomings, this is a valuable study of an elite whose collective mentality left individuals ill-prepared to confront realities. What else could be said of leaders who really thought that Léon Blum's

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policy of nonintervention in the Spanish Civil War was pro-Loyalist?

NATHANAEL GREENE
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JEAN LACOUTURE. *Pierre Mendès France*. Translated by GEORGE HOLOCH. New York: Holmes and Meier. 1984. Pp. viii, 486. Cloth $39.50, paper $19.50.

PIERRE MENDÈS FRANCE. *Oeuvres complètes*. Volume 1, *S'engager, 1922–1943*. Paris: Gallimard. 1984. Pp. 837. 195 fr.

Although Jean Lacouture's biography was first published in the year preceding the death of Pierre Mendès France, it provided in all essentials a complete survey of the life of one of France's most hated and admired statesmen. Testimonies to the enduring loyalty of his associates and supporters are provided by the heartfelt tribute paid by President François Mitterrand in 1982 (which is appended to the English translation of Lacouture's biography) as well as by the publication of the first volume of his complete works, covering the years 1922–43, under the auspices of the Institut Pierre Mendès France. The second volume, covering the years 1943–54, was published in mid-1985, and four more volumes will deal with the period from his memorable government of 1954–55, when he came to be known by his initials, PMF. The hatred may have diminished over the years, as those who had felt threatened by PMF realized that, except as a symbol, he was a spent force. Throughout his political life, however, Mendès France suffered from the unscrupulous smears of the anti-Semites and those who feared this authentic and intransigent if idiosyncratic republican, radical, and socialist.

What emerges from both Lacouture's biography and the volume of PMF's early writings is the influence on him of three twentieth-century French statesmen—Raymond Poincaré, León Blum, and Charles de Gaulle. Firstly, there was Raymond Poincaré, to whom PMF devoted his doctoral thesis (written at the age of twenty-one), an abridged version of which is republished in the first volume of the *Oeuvres*. He admired the skill and courage with which Poincaré, as the head of the government of 1926–29, stabilized the value of the French currency; from this episode PMF learned the lesson that a policy of financial rigor was the indispensable foundation of a successful economic policy. In his 1928 thesis, however, Mendès France criticized the industrial and social consequences of the deflationary policy pursued by Poincaré, and in early 1937 he was one of the earliest Frenchmen to read J. M. Keynes's *General Theory of Employment, Interest, and Money* and to seek to implement a Keynesian cheap money policy. He did so as undersecretary of state for the treasury in the second Blum Popular Front government (which only lasted from March 13 to April 10, 1938) in which Blum, nominally minister of finance, left matters to his young colleague. The bill that Mendès France prepared in conjunction with Georges Boris (the author of an admiring 1933 book, *La révolution Roosevelt*) sought the powers necessary to stop the flight of capital by imposing exchange controls, supported by price controls and a capital levy. It predictably met with insuperable Senate opposition and played a major part in the fall of the second Blum government. The purpose of this "plan" appended to volume 1 of the *Oeuvres* was not merely to revive the French economy but also to provide the resources for an active policy of rearmament against the Nazi menace. Here the link with de Gaulle is already clear, even before Mendès France escaped to England during the Second World War, later serving in de Gaulle's provisional government as minister for the national economy. But de Gaulle, like Blum, was not really interested in, or capable of, grasping the combination of Poincarian and Keynesian analyses that subsumed PMF's proposals for postwar economic reconstruction. Yet this was precisely what made PMF's sustained and unyielding critique of Fourth Republic economic policy so inspiring to the young technocrats who have been so influential in the running of the French economy.

The editing of Mendès France's writings is unobtrusive, providing the bare minimum of contextual information to the books, articles, speeches, and correspondence that are published. Lacouture's biography essentially confirms the received view of PMF and is less innovative than the first volume of his study of de Gaulle. He writes with a scrupulous respect for the truth that is worthy of his subject. Despite excessive misprints and occasional inaccuracies and infelicities of translation, the English version is readable. It allows us to follow the career of a man who remained a Young Turk to the end of his life, refusing to adjust to the modern political party and unwilling to resort to the methods used by de Gaulle to recover power. They were both heroic leaders, but PMF was brought down by the Algerian settler lobby that de Gaulle was first to use as his stepping stone to office and then to discard and demolish. Although Lacouture exaggerates PMF's rationalism, Mendès France was always inhibited from behaving with the cynical pragmatism that allowed de Gaulle to leave a much more enduring mark on late twentieth-century France. These volumes are useful preparations for more searching studies that will assuredly come.

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JOSÉ LUIS GARCÍA DELGADO, editor. *La España de la Restauración: Política, economía, legislación y cultura.* (I Coloquio de Segovia sobre Historia Contemporánea de España.) Madrid: Siglo Veintiuno. 1985. Pp. ix, 545.

This is a collection of papers presented at the first colloquium on the contemporary history of Spain sponsored by the Universidad Internacional Menéndez Pelayo in Segovia in April 1984. Given the general title "Economic Backwardness and Social Conflict, 1875–1914" by its chairman, Manuel Tuñón de Lara, the conference included both well-established and junior scholars committed to the "scientific" exploration of topics either previously neglected or encrusted in clichés and political prejudice. Nearly all the papers reproduced here amply fulfill the aspirations of the conference participants.

Significantly, in preparing the papers for publication, the editor dropped the original title of the conference in favor of the descriptive title that appears above. In fact, the general thrust of these articles points toward modification of the accepted view of Restoration Spain as illiberal, traditional, and stagnant. Most of these papers attempt to analyze the system in its own terms, from inside its own premises and structures rather than from without. In all, they suggest a healthy tendency among younger Spanish historians to examine processes as well as results and to provide explanations rather than simple condemnations. Their conclusions revise traditional stereotypes and open new areas for further research.

Because it is impossible in a short review to comment exhaustively on a heterogeneous collection of twenty articles, I will focus on two sections that seem to challenge prevailing interpretations most thoroughly. In the section on Restoration politics, Antonio Calero Amor and Mercedes Cabrera show that many contemporaries, including Antonio Maura, thought the parliamentary monarchy was capable of evolving toward democracy and worked to achieve it. As María Victoria López-Cordón notes, the clerical Right, despite the privileged position of the church, viewed the regime with suspicion, perceiving perhaps more clearly than later observers its basic liberalism and secularism. Although acknowledging the real limitations of the monarchy, these reevaluations nevertheless present it as a dynamic political system not inherently incompatible with democracy, a point of view that may be more congenial in Spain since the most recent restoration of the Bourbon monarchy.

The section on the Spanish economy contains excellent interpretive articles by Gabriel Tortella and Antonio Bernal that review the debate on Spain's failure to keep pace with the rest of Europe, together with several specialized studies that address some of the many unresolved questions in Spanish economic history. Their conclusions are complex, sometimes contradictory, and not easily summarized. Nevertheless, the general picture that emerges is not one of "feudal" stagnation but rather one of steady if incomplete economic modernization, limited state responsibility for either success or failure, and, above all, an underlying capitalist rationality—that is, profitability—that discouraged risk taking and innovation. The greatest failure of the state lay in its credit and protectionist policies, which delayed the agricultural modernization that was the key to more rapid growth for the economy as a whole; despite Spain's position on the "periphery," liberalization would have promoted a healthier economy in the long run. Overall, the articles suggest the inapplicability of the British model to Spain and should stimulate further research into how the Spanish economy worked, not why it "failed."

CAROLYN P. BOYD
University of Texas

AARON L. KATCHEN. *Christian Hebraists and Dutch Rabbis: Seventeenth-Century Apologetics and the Study of Maimonides'* Mishneh Torah. (Center for Jewish Studies, Harvard Judaic Texts and Studies, number 3.) Cambridge: Harvard University Center for Jewish Studies; distributed by Harvard University Press, Cambridge. 1984. Pp. xvii, 391. $28.00.

This work explores a much-neglected subject, the views of some of the Dutch Hebraists and their relationships to the Amsterdam rabbis, especially Menasseh ben Israel. Much of the book examines Dionysius Vossius's Latin translation of Maimonides on idolatry. Modern Hebrew scholars usually ignore Christian Hebraic writers and Christian interest in Maimonides' thought in the seventeenth century. Aaron L. Katchen's book treats some Dutch Christian Hebraists seriously, examining their training, interests, and purposes.

From the early seventeenth century onward, Christian Hebraists realized that Judaism was not the religion described in the Bible but the application of later Jewish thought, found in the *Mishna*, the *Talmud*, and rabbinical commentaries, to living Jewish communities. The Hebraists studied and translated some of this postbiblical material. Among their purposes was understanding Judaism in order to convert seventeenth-century Jews.

Katchen's study focuses on a limited portion of the available material; he examines a group of liberal Calvinist Hebraists who learned from and were in contact with Menasseh ben Israel. Because of the narrow focus, the larger context of the millenarian concern with preparing for the imminent conversion of the Jews is missed, as is Menas-

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seh's deep involvement with the millenarians. Unfortunately, Katchen's study is already outdated because so many new findings have appeared. Constantine L'Empereur did study with Menasseh. Menasseh collaborated with the millenarians Adam Boreel and John Dury in publishing an exact, vocalized edition of the *Mishna* in Hebrew, and Rabbis Templo and Abendana worked with the millenarians on Spanish and Latin translations of it. The rabbis knew that Boreel, supposedly the greatest of the Dutch Hebraists (but who is not mentioned in Katchen's book), expected that Jews would want to become Christians from reading this edition of the *Mishna* and that Christians would learn to make Christianity less offensive to the Jews. Not mentioned by Katchen is Dury's proposal in 1649 to establish a college of Jewish studies in London with Menasseh and Boreel as professors. Menasseh participated in and became a spokesman for a genuine effort to unite Judaism and Christianity into a common expectation about the future that placed minimal emphasis on differences about first-century history (a view developed by Isaac La Peyrère and advocated by several European millenarians). Menasseh may have started on this road from his contacts with Simeon ben Jacob Günzburg (whom Katchen's work emphasizes), who endorsed Menasseh's early views. The development of Menasseh's interactions with the millenarians is hardly mentioned, even though it explains much about his views and his ability to function as an equal in the Christian Republic of Letters.

Recent findings of H. Méchoulan, E. van der Wall, and myself are unknown or ignored by the author. H. P. Salomon's discovery of Menasseh's family background and of his birth in La Rochelle is not mentioned. Regrettably, David S. Katz's *Philo-Semitism and the Re-Admission of the Jews to England* is listed in the bibliography with the statement, "Not consulted." It would have required the rewriting of much of Katchen's last chapter, for Katz's book, which appeared in 1982, made all previous discussions of Menasseh's involvements with the English millenarians obsolete. Further material from the Leiden Symposium of 1985 on Jewish-Christian relations and from the Colloquium on Menasseh (Israel, 1985) calls for much rethinking of Katchen's presentation.

With what Katchen presents, as well as what is now known of the broader context of millenarian concern with postbiblical Jewish writing, it will be possible to explore further this rich area in which Jewish and Christian scholars came so close in scholarly work and to some extent in theology. Vossius's translation of Maimonides became central for many thinkers, including Theophilus Gale and Isaac Newton. Perhaps at no time since the first century had Judaism and Christianity interacted so vigorously as in Holland and England during the seventeenth century. To appreciate what happened and why, all aspects must be studied. Katchen has done an excellent job on a small portion of the material but has taken it to be the whole story. Placed alongside the exciting new finds about both the Christian Hebraists and the rabbis of the time, it will help enrich our understanding of a quite amazing period in modern intellectual history.

RICHARD H. POPKIN
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AUGUST WIEMANN ERIKSEN. *Eksportrådgivning i Danmark, 1910–1921: Privat eller statslig opgave? Striden i erhvervsorganisationer og centraladministration om oprettelse af et statsligt oplysningsorgan for eksporterhvervene* [Export Information in Denmark, 1910–21: A Private or State Obligation? Conflict in Business Organizations and Governmental Agencies over the Establishment of a State Information Organ for Export Trade]. (Administrationshistoriske Studier, number 9.) Rigsarkivet: G. E. C. Gads. 1984. Pp. 198. 134.20 KR.

This study describes the conflict from 1910 to 1921 between Danish industries and government agencies over the establishment of a governmental bureau to assist the various export interests. The central issue was whether exports should be the responsibility of private companies or of the government. Beyond this, August Wiemann Eriksen delineates the conflict between the Ministries of Trade and Foreign Affairs, both of which sought to act as the exclusive agency to serve Danish export interests. Finally, his purpose is to show that the decision to assign full responsibility to the Ministry of Foreign Affairs was the "result of significant conflicts between various export groups attempting to promote their special export interests" (p. 15).

During the first decade of the century the most powerful business group was the Wholesalers' Society, whose members were instrumental in establishing the Ministry of Trade in 1908 to handle their interests. After 1910 it became clear that a new interest group had come to the fore—the large urban industrialists with their Industrial Council. Tables in the appendix illustrate the relative decline of craft items and agricultural products and the rise of industrial goods produced from the 1890s to 1921. Table 3 makes clear that, although the value of farm commodities was still far ahead of industrial goods between 1910 and 1921, the growth rate of the latter was definitely more spectacular. The information that the wholesalers and industry needed for export purposes was quite disparate. The Ministry of Trade served primarily the wholesalers. The focal point for competition between wholesalers and

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industrialists became the Ministry of Foreign Affairs, partly because of its importance during the war years. With their increased role in Denmark's national economy by the end of World War I, together with support from conservative politicians in parliament, wholesalers and industrialists were able to centralize all export information in the Foreign Ministry's Board of Trade by 1921. What this meant was that one agency, the Board of Trade, "could now systematically take on the responsibility of providing assistance to Danish exporters according to their individual export needs" (p. 176). To Eriksen's credit, he places the Danish development in the larger European and world setting.

This is the ninth monograph in a series of ten studies in Danish government administration from the seventeenth to the twentieth century. The format is attractive, with large print, photographs that are well explained, and tables, notes, sources, and bibliography at the end. This book will be of interest only to a small group: those concerned with the interaction of private and governmental bureaucracies in the early twentieth century, when large-scale industry was coming to the fore at the expense of older production sectors such as crafts and agriculture. The sentences are Germanic in length, and compound words abound. Eriksen's survey of the literature is good, and he has justified his research.

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WOLF D. GRUNER. *Die deutsche Frage: Ein Problem der europäischen Geschichte seit 1800.* (Beck'sche Schwarze Reihe, number 267.) Munich: C. H. Beck. 1985. Pp. 217. DM 19.80.

Is there really such a thing as the "German question"? Many Americans and even some Europeans may have their doubts, but the Germans are generally convinced that the "German question" does indeed exist, that they have been wrestling with it throughout their history, and that the security not only of their country but of the entire Continent as well depends on its solution. Wolf D. Gruner, who belongs to the postwar generation of German historians, has now written a brief account of this vexing question from the Congress of Vienna to the present. His tone is temperate and restrained; he rejects the contention of earlier nationalist writers that Germany's disunity was primarily the result of its neighbors' envy and greed. He argues rather that the Germans have in the past failed to recognize the special role their country must play in international affairs, a role dictated by geographic position and political situation. German unity, in other words, has to be achieved in a way that satisfies the national aspirations of Germans without threatening the legitimate interests of non-Germans.

The book opens with a lengthy discussion defining the nature of the "German question." Then come chapters on the German Confederation, the revolutions of 1848–49, and the *Reichsgründung*. Gruner tends to exaggerate the hostility of foreign states to German unification and to underestimate the strength of particularism among the Germans themselves. Yet he concedes that German nationalists misjudged the possibilities for political consolidation, achieving their objective in defiance of rather than in harmony with the other nations of the Continent. The German empire, moreover, used its power unwisely, failing to perceive that a mediating role between Eastern and Western Europe was essential for its security. The result was defeat, dictatorship, and partition. Now the task is to achieve German unity once again, but this time within a framework of European unity.

This argument is presented with moderation and scholarly objectivity. Its tone is quite different from that of most previous writers on the "German question." But Gruner's book is not only a work of historical exposition; it is also a tract for the times. It reflects the mood of present-day liberal Germany, recovered from the physical and psychological wounds of the Second World War, seeking to re-create the national unity that totalitarian hubris destroyed. The author is subtly but unmistakably critical of both superpowers, of the Americans perhaps even more than the Soviets. To him the partition of Germany in 1949 was a tragedy. The sovereignty that the Federal Republic acquired in 1955 appears here in quotation marks, as if it were not quite the real thing. And the chance to establish a united though neutralized Germany, which Stalin seemed to favor in 1952, was lost because of Washington's obduracy. But it is not too late to correct those mistakes, Gruner concludes. A federal union of the two Germanys within a peaceful and integrated Europe—that is the best solution to the "German question." That many Germans share this view is clear; how many non-Germans will accept it is another question.

THEODORE S. HAMEROW
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JOSEF MOOSER. *Ländliche Klassengesellschaft, 1770–1848: Bauern und Unterschichten, Landwirtschaft und Gewerbe im östlichen Westfalen.* (Kritische Studien zur Geschichtswissenschaft, number 64.) Göttingen: Vandenhoeck und Ruprecht. 1984. Pp. 521. DM 98.

In spite of all that can be and has been said against it, the term "protoindustrialization" lives on. It has

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stimulated important new work by, among others, Wolfgang Mager and Jürgen Schlumbohm and now forms the backbone of Josef Mooser's fine history of rural social relations in eastern Westphalia, from 1770 to 1848. Historians have frequently discussed this region as an important German example of a commercially successful rural linen industry eventually ruined by factory-manufactured linens and cotton. Mooser's study nevertheless adds considerably to our understanding, not only of rural eastern Westphalia (especially Minden-Ravensburg) but also of protoindustrialization generally. This brief review, however, can do no more than simply mention a few reasons why Mooser's book is worth reading.

The author's sympathy and attention is concentrated mainly on the growth and development of a particular variant of rural proletariat, the *Heuerlinge*, whose livelihoods depended on both the linen trade (for which they spun and wove) and peasant landowners (for whom they worked in exchange for rented land). Mooser's explanation for the growth of this social class, like that of other historians of protoindustrialization, is basically Malthusian: protoindustrial earnings make possible earlier marriages, free of hindrances related to peasant land inheritance, and reproduction does the rest, resulting in the relative growth of landless families in the region. Though essential, this part of the story is not particularly striking. What makes Mooser's account interesting are two other features. First, his stress on relations between peasant landowners and landless *Heuerlinge* as the key social and economic nexus not only conveys a more plausible picture of contemporary social relations in this rural setting but also (implicitly) fills a gap in the economic models of protoindustrialization. He shows clearly that peasant landowners were major beneficiaries of protoindustrial expansion and that their accumulation of wealth paralleled the rising ratio of nonagricultural to agricultural employment. Second, his skillful use of sources brings us beneath the stratosphere of abstractions of class and nearer to the daily lives of these Westphalian peasants and *Heuerlinge*, thus enabling us to understand better their aspirations and behavior in cultural terms (the author's discussion of consumption as "luxury" is one positive example). This is well-executed "history from below," reminiscent of Rudolf Braun's brilliant, pioneering work in the 1960s.

R. TILLY
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DIETER DÜDING. *Organisierter gesellschaftlicher Nationalismus in Deutschland (1808–1847): Bedeutung und Funktion der Turner- und Sängervereine für die deutsche Nationalbewegung.* (Studien zur Geschichte des Neunzehnten Jahrhunderts, number 13.) Munich: R. Oldenbourg. 1984. Pp. 357.

During the forty years prior to the revolutions of 1848–49, associations of gymnasts and singers were the main organized mass movements giving expression to German national feelings. Heretofore, although their significance has by no means been overlooked, they have not received the intensive and comprehensive treatment that Dieter Düding has now provided with this book. Turning his back on anything suggestive of "a history of events" or a personalized history of ideas in the style of Friedrich Meinecke, Düding has applied an "empirical-systematic" comparative method centered on such topics as organization, communication, social relations, and rituals. He cannot be faulted for deficient research. East and West German archival materials, contemporary speeches, pamphlets, song books, memoirs—all have been thoroughly investigated. The documentation is, if anything, excessively full.

The subject breaks down into two phases. During the first, which lasted from 1808 to 1820, organized nationalism found expression mainly in the associations of gymnasts and their occasional allies, the patriotic fraternities of university students. Following the Carlsbad Decrees in 1819, however, the various German governments effectively drove the *Turnvereine* out of existence. Meanwhile, associations of male singers with a repertory of patriotic and folk songs had achieved substantial popularity, particularly in southwest Germany. In the second phase, after 1820, these *Sängervereine* proliferated in other areas, organizing on a regional basis and holding ever-larger festivals celebrating all-German themes. The gymnasts were slower to revive, but during the 1840s they picked up much momentum, reaching a climax in 1847 with several great festivals where, in speeches supplementing the gymnastic displays, the *Turner* gave notice, sometimes in violent language, that they wanted change.

In a particularly useful chapter, the author lays out in detail the political substance of the songs and oratory. He has put himself in a methodological strait jacket, however, by largely ignoring the context within which these activities occurred and by reducing interpretation almost to nil. Furthermore, by depersonalizing his account so thoroughly, he has drained the life out of it. A case in point is the treatment of Friedrich Ludwig Jahn. The author makes clear that the early gymnastic movement was to a very large extent the creation of Jahn, who provided its ideology and was its organizing genius. No other person rivaled him in transforming German national feeling into an organized movement, but Düding thinks German historians have erroneously given Jahn a bad name (and Jahn has certainly fared no better from non-German historians). Dü-

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ding himself, however, never addresses the problematic aspects of the *Turnvater*, nor does he deal with him in a comprehensive way. One will get a far better idea of what is involved in reevaluating Jahn from R. Ray Lutz's brief article,"'Father' Jahn and His Teacher-Revolutionaries" (*Journal of Modern History*, on-demand supplement [1976]) than from this book. Incidentally, every item in Düding's bibliography, with one exception, is in German; one would have supposed, for example, that George L. Mosse's *Nationalization of the Masses* (1975) might have merited a nod.

To sum up, this is a workmanlike but unimaginative study, strong on facts and figures organized from a social science perspective.

PAUL R. SWEET
Michigan State University

SHLOMO AVINERI. *Moses Hess: Prophet of Communism and Zionism*. (Modern Jewish Masters, number 1.) New York: New York University Press. 1985. Pp. xii, 266. $22.50.

There are not many occasions when a writer finds an expositor as temperamentally well suited to him as Shlomo Avineri is to Moses Hess. Avineri is already familiar as the author of highly respected and almost frighteningly lucid books on the political thought of Marx and of Hegel, and it is only natural that he should now turn to one of the shadowy transitional figures who stood between them. But Hess was more than a leading if eccentric member of the Young Hegelians; he was also assertively Jewish in his national identification, and the author argues persuasively that Hess's thought must be seen in terms of an unsteady movement between the poles of proto-communism and proto-Zionism. The same, one suspects, can be said of Avineri himself.

The tension between the universalistic promises of communism and the national allegiances of the proletariat has been the most intractable problem in the history of socialism, and on this crucial issue the texts of Marx and Engels prove singularly inadequate. Although this tension was not only perceived by Hess but also became thematic for his whole intellectual career, he has never been accorded more than the marginal status of a theoretical "precursor" who pointed Marx and Engels toward the promised land of proletarian democracy he himself was not destined to enter. The so-called true socialism of Hess is flung off by the authors of *The Communist Manifesto* like an outgrown overcoat.

In addition to the unlucky fate of having been criticized by Marx himself, Hess's reputation has suffered at his own hand. An autodidact, he possessed a fiery spirit and indisputable intellectual gifts but not the patience for methodical research and disciplined argumentation. His writings between 1837 (*Die heilige Geschichte der Menschheit*) and 1862 (*Rom und Jerusalem*) emit flashes of philosophical brilliance and present positions with a prescience not approached by even the most farseeing of his contemporaries, but they are developed, shall we say, in the prophetic mode—unsystematic, disjointed, rambling, and sometimes a little hysterical.

Avineri sets himself the task of restoring an appropriate measure of intellectual credibility to Hess, without overstating the case. What emerges from his account is a man who heard deep and ominous rumblings beneath the surface of a modernizing society, who called out a prophetic warning to European Jews to be wary of the universalistic promises of the Enlightenment but who could not ground his case in a way that would enable it to take root. I would have appreciated some greater resonance from the political climate of the Europe that Hess observed and absorbed, and I often felt that Avineri measured his subject's intellectual stature too regularly by the standard of Marx. His sympathetic and thereby provocative treatment is nevertheless a welcome contribution to an understanding of the origins of socialist and Zionist thought.

This book is the first volume of a new series on modern Jewish masters; subsequent volumes will treat such diverse writers as Martin Buber, Sholem Aleichem, and Hermann Cohen. Although we must feel some ambivalence toward yet another series of profiles devoted exclusively to modern *male* masters, Avineri has gotten it off to a good start.

BARRY M. KATZ
Stanford University

HELMUT TROTNOW. *Karl Liebknecht, 1871–1919: A Political Biography*. Hamden, Conn.: Archon. 1984. Pp. viii, 276. $29.50.

Helmut Trotnow takes aim at Karl Liebknecht's reputation (common to friendly and hostile historians) as a political activist or agitator without coherent ideas—"a politician without a mind" (p. 6). Liebknecht, he argues, did have a consistent pattern of ideas and goals, if not exactly a theory, that motivated his activity both as agitator and parliamentarian before 1914 and as a lone rebel from then to his death in 1919. But these ideas were not Marxist. Liebknecht was driven to overcome injustice and inhumanity, promote socialism as an emancipation movement, and bring enlightenment to the workers. His mind did not work dialectically; he understood change through a mixture of voluntarism and determinism. And, for all his radical exhortations, his idea of mass mobilization for change was more reformist than revolutionary and did not entail violence. The peculiar circumstances that led

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to his close alliance with Rosa Luxemburg and to his death as a Communist leader left a false impression of what Liebknecht really stood for.

This vision of Liebknecht, not entirely new but argued at length here for the first time, makes a good deal of sense. The Liebknecht to whom historians have previously condescended has not been fully plausible; we have had to consider too many of his actions as merely heedless or psychologically abnormal. Now we can see an intelligible cast of mind and some coherence and continuity in his understanding of the labor movement's mission. But the portrait needs a much better background than we find in this book. The author seems unaware that the orthodox Marxism of that day is commonly seen as deterministic and nondialectical, with reformist, emancipatory goals—the same things he finds in Liebknecht. Liebknecht's differences from, say, Karl Kautsky need to be explored. The other obvious contrast—the innovative Marxist thinking of Rosa Luxemburg—is presented in a sketchy, flat, and unconvincing way. Likewise, the political background is unsuccessfully drawn. Liebknecht was a lone wolf, repeatedly driven by a special sense of political morality into isolated positions. These obligatos to the sober Social Democratic choral performance often won him approving public attention, but they left his fellow politicians skeptical or scornful. He was not taken seriously. The book tries to make us take him seriously, with some success, but does not help us understand why so many of his contemporaries could not do so.

Unfortunately, the text, written by the author in a language not his own (it also appeared in German in 1980), is deficient in clarity, directness, and balance. Argument and explanation and even narrative often give way to extended quotation and paraphrase from its subject's writings. Someone should have edited the confusing run-on paragraphs (several are two full pages long; at least one is almost three). Apparently nobody corrected proofs at all. Getting at the book's useful insights is a chore.

DAVID W. MORGAN
Wesleyan University

LOUIS L. SNYDER. *Diplomacy in Iron: The Life of Herbert von Bismarck*. Malabar, Fla.: Robert E. Krieger. 1985. Pp. xii, 235. $16.50.

Fate lays a special burden on great men's sons. Developing strong personalities and careers of their own is difficult. One thinks of Franklin D. Roosevelt's sons, of the younger Randolph Churchill, and of Herbert Bismarck. The eldest son of the founder of the second German empire is an especially interesting figure, for not only did he become an indispensable part of his father's political machine in its last phase but, in his complexities and contradictions of character and outlook, he also exemplified and dramatized the problems of the fin-de-siècle generation in Germany. The Bismarck Archive in Friedrichsruh contains ample source material, primarily diaries and letters, for a full-scale biographical study of this man in the context of his time. In 1964 Walter Bussmann edited a six-hundred-page collection of Herbert's political correspondence, including a thoughtful introductory chapter analyzing Herbert's personality and career. But even a large selection has to leave out much, and one with a political orientation cannot include many of the personal communications—such as the bulky correspondence with Baron von Scheel-Plessen, an intimate friend—that are especially valuable to a biographer.

A serious, full-scale biographical study is thus both possible and desirable, but this book by Louis L. Snyder is not it. Snyder's light, brief survey of Herbert's personality and career concerns itself more with the father than with the son. It is based on memoirs, biographies, printed correspondence, and monographs, mostly published before World War II. The Bussmann collection is listed (twice, once erroneously), but apparently neither it nor the Friedrichsruh archive was used. Herbert is described as a "Jeckyll and Hyde" personality because of his capacity for both great charm and arrogant brutality, and his heavy drinking is stressed. In general, Snyder's analysis of both character and career not only does not go beyond but also does not come up to Bussmann's essay.

The title of Snyder's book is unfortunate, since flexibility, not rigidity, is the essence of diplomacy, especially Bismarck's; flexibility, in fact, characterized Bismarck's career in general. The "Iron Chancellor" myth was natural for the brash new Second Empire before World War I and is best exemplified by the colossal granite statue of Bismarck in medieval armor overlooking the harbor at Hamburg. Since World War II, German scholars have universally repudiated such an approach, and it is not entirely clear why American historians would want to perpetuate it. Snyder also makes a number of minor errors, portraying the journalist Moritz Busch, for example, as an under–state secretary (p. 76) and reviving the old myth, long since laid to rest, of Friedrich von Holstein's "spying" on Harry von Arnim, his superior in Paris. The book is printed in an extra-large sans serif, and in the one illustration a portrait of Herbert is overwhelmed by an oversized reproduction of a well-known photograph of his father wearing a helmet.

J. ALDEN NICHOLS
University of Illinois

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KLAUS SCHWABE and ROLF REICHARDT, editors. *Gerhard Ritter: Ein politischer Historiker in seinen Briefen.* (Schriften des Bundesarchivs, number 33.) Boppard a/R.: Harald Boldt. 1984. Pp. xvii, 770.

This book leaves the reader with mixed feelings. He is bound to admire Klaus Schwabe and Rolf Reichardt's scrupulous scholarship, particularly Schwabe's 170-page introduction that combines respect for truth with devotion to the memory of his teacher. But throughout the perusal of more than 800 pages this reader also kept asking himself whether the results of this enterprise justify the effort. Schwabe and Reichardt began their labor at the request of Gerhardt Ritter's widow, and they now claim to have made a scholarly contribution to the continuing debate on Germany's unique road through history and to have given us a close-up of a chapter in German historiography. The modesty of that assertion does not appease this critic.

The history of every nation constitutes a *Sonderweg* of sorts, often turned by such debates into a myth of mission. If this study contributes to the German dimension of a universal paranoia, should such a contribution be welcomed? Adding to the reviewer's concern is a phrase in the foreword avering that Ritter's work "includes all of modern and contemporary history." Actually, Ritter's *oeuvre* stays consistently with German history since the Reformation. His few but sweeping judgments of the history of other European nations display appalling ignorance, as is witnessed by his verdict on the Third Republic, which, to him, viewed "freedom merely as high living and indiscipline" (p. 553).

It is true that Ritter himself occasionally deprecated scholars who continued "to rummage again and again through *specialissima* of Prusso-German history" (p. 541). But such pangs of conscience proved fleeting, elicited in this instance by Dietrich Gerhardt's *Alte und neue Welt in vergleichender Geschichtsbetrachtung* (1962). His own life and work present a constant struggle to come to terms with the German past, an endeavor involving endless explorations of Prusso-German archivalia, from his debut as research assistant on Hermann Oncken's biography of Rudolf von Bennigsen (1910) to the fourth volume of *Staatskunst und Kriegshandwerk.*

This brings us to the authors' second claim: that they have written a chapter in the history of German historical scholarship. They have indeed illuminated the battles German historians had to fight with contemporary history, and themselves, to retain a firm footing on constantly shifting ground. Ritter was born during the hundred days of Frederick III and survived the Adenauer era. He entered military service during World War I, already established as an authority on modern Prussian conservatism. During the next quarter century he added to his reputation with studies on Luther, a new edition of Bismarck's memoirs (with Rudolf Stadelmann), the two-volume biography of Baron Stein (1931), and a "historical profile" of Frederick the Great. After World War II, he wrote the biography of Carl Goerdeler, his great work of atonement, and *Staatskunst und Kriegshandwerk* (1954–68). These summits in a bibliography of 472 items stamp Ritter as a major historian of his generation and of that generation's struggle with repeated disasters.

The authors have contributed to the elucidation of this painful and complicated process by their choice of letters, which document Ritter's role in the controversies, both historic and historiographic, arising from his commitments. For the purposes of this review, three examples must suffice. First, introduction and documents detail Ritter's dedication to historical truth while recording moments of despair when he doubted that anyone could write "scientific history" (p. 541). Second, the letters allow us to appreciate every step of Ritter's tortuous path between accommodation and opposition to the Nazi regime, including letters from concentration camps and prison. Third, the book exposes the Freiburg historian's baneful role, as scholar and as partisan, in the controversy following the publication of Fritz Fischer's *Griff nach der Weltmacht* (1961). The result is no eulogy. On a personal plane, Ritter's abhorrence of Hitler ultimately placed his own life in jeopardy. But his scholarly contribution to the chronicle of the German Resistance must now be judged in the light of Leonidas Hill's essay in *Central European History* (14 [1981]: 369–99), which invites us to go beyond the parochial, Prussophile conclusions of the Ritter-Rothfels school.

Belief in Ritter's methodological probity is shaken likewise by the book under review. The introduction admits that "the question of his opponents, whether as a scholar he held to the severe criteria by which he judged the investigations of his colleagues, . . . was not unjustified" (p. 168). It goes on to explain that Ritter's intervention against government financing of Fritz Fischer's trip to the United States in 1964 was prompted by his reaction to a work that "seemed to jeopardize the political and historical labor of a lifetime" (p. 151). Although that episode exposes Ritter as an unscrupulous and devious adversary, it was not without precedent, as is indicated by his opinion on Eckart Kehr's manuscript on Stein (pp. 237–47), since lost, and on another monograph challenging a major work of his, as well as by his contention elsewhere that Kehr was an "*Edelbolschewist* . . . who should seek habilitation in Russia rather than in Königsberg" (p. 237).

This book portrays an important historian succumbing on occasion to unscholarly passion and a Christian lacking in charity, both afflictions from which few historians can claim immunity. To the

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credit of the authors, they included these revealing pieces in their collection. What is more important in the long run, however, is the weakness of the case against Fischer (and Kehr), Ritter's chief preoccupation in his productive old age. We know today, thanks to the Riezler diary from which Ritter expected a measure of vindication, that, by Ritter's criteria, his own estimate of Bethmann-Hollweg was farther off the mark than Fischer's. We also know since the publication in 1967 of Hermann Kantorowicz's *Gutachten zur Kriegsschuldfrage 1914*, originally completed in 1927, that Fischer's theses, contrary to Ritter's contention, were not particularly new and that broad scholarly and official dedication to *raison d'état* postponed the revelation of these uncomfortable truths for forty years. Ritter had nothing to do with that. But, as one studies his reaction to Fischer, one wonders how he would have reacted to Kantorowicz's book. Would he not have attacked its author as venomously as he did Fischer? This leaves us questioning the degree to which Ritter's work remains valid. The authors remain convinced of its significance; the reviewer sees a crumbling edifice.

HANS A. SCHMITT
University of Virginia

GEOFFREY COCKS. *Psychotherapy in the Third Reich: The Göring Institute*. New York: Oxford University Press. 1985. Pp. xii, 326. $24.95.

This valuable book addresses one of the most surprising anomalies of the Third Reich: at the height of Nazi persecution under the "psychopathic god," psychotherapy attained a professional status and support from the state unrivaled in Germany before or since. It was able to do so largely because of an institute established by a cousin of Reichsmarschall Hermann Göring.

The Nazis distinguished between psychoanalysis and psychotherapy. They condemned Freudian analysis because it was "Jewish science" and also because they suspected, quite rightly, that psychoanalysis was inherently antiauthoritarian. By contrast, psychotherapy was tolerated and even encouraged by the Nazis because it was seen as harmonizing with *Gleichschaltung*, Hitler's program of "integrating" the individual by subordinating him to the community. Furthermore, Hitler and Goebbels, themselves shrewd amateurs, had a pragmatic interest in discovering ways psychology could be employed in manipulating the masses. Himmler and racists of his ilk approved of the institute because they believed that psychology would help them probe the unfathomable resources of the *volkisch* soul.

Freudian psychology, though officially condemned, managed to survive precariously in the institute. Visitors were greeted with the arresting image of portraits of Adolf Hitler and Sigmund Freud engaged in a staring contest from opposite walls of the foyer. The contest continued until 1938 when the Nazis decided that the Führer had won. Freud's portrait, still unblinking, was removed. But his influence remained; for psychotherapists realized that, despite the undoubted Jewishness of the origins, Freud's scientific contributions were essential to their work.

Matthias Heinrich Göring (1879–1945) was, in virtually all respects, the opposite of his flamboyant cousin. A shy and gentle person with a stammer and grandfatherly beard, he dressed in somber tones and invariably carried with him a Lutheran Bible. He managed, somehow, to accommodate his Christian piety to Nazi ideology and announced that the task of his institute was "to force all abilities from a person, not for his own sake, but in service to the *Volksgemeinschaft*" (p. 58). He ordered his therapists to study *Mein Kampf*. His cooperative attitude and his influential name kept him in the good graces of the party.

M. H. Göring, more than anyone else, was the preserver of psychotherapy in Nazi Germany: he protected patients from Gestapo investigation, and he insisted on maintaining professional standards of training and therapy throughout the Hitler period. But he also helped destroy the careers of Jewish therapists whose gifts far exceeded his own, and he collaborated eagerly with the Nazis. His institute offered its services to various party organizations, treating members for impotency and homosexuality until Hitler ordered the death penalty for all homosexuals. The institute also contributed ideas for psychological warfare and tried to find scientific justification for vicious racist nonsense. Göring and his professional colleagues raised no objections to Hitler's infamous "euthanasia" program, which murdered over two hundred thousand people, most of whom were mental patients.

Geoffrey Cocks devotes inordinate space to accounts of the careers, theories, and rivalries of dozens of forgettable psychotherapists connected with the institute. He carefully catalogues defunct psychological associations and traces the convoluted story of their competitions for favor and power. All this bespeaks impressive research and increases the value of the book for reference purposes, but the general reader becomes wearied with excessive detail. Yet items of interest emerge. We learn, for example, that the institute prospered under Göring's leadership, the number of therapists increasing from 128 in 1937 to 240 in 1941 (of whom 100 were medical doctors); that the institute offered a variety of treatments including psychoanalysis,

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group therapy, and hypnosis; and that training was rigorous. Regrettably, the discussion of training and therapy is limited to a few pages that deal only with psychopathology within the Göring Institute. Thus, the enticing promise of the book's title is not really fulfilled; we are given detailed institutional history rather than a broad treatment of psychotherapy in the Third Reich.

As Cocks forthrightly notes, historical evaluation of the institute must weigh the personal motives and the actions of individual therapists against institutional achievement. And yet, however suspect the motivation of its members, the accomplishments of the Göring Institute were not unimpressive. Although no science, art, or humane study could flourish in the poisoned soil of the Third Reich, this book demonstrates that psychotherapy in its many forms did survive, that it actually made significant advances toward professional and institutional acceptance during the years from 1933 to 1945, and that these advances were carried over into the Federal Republic. The irony is remarkable: psychotherapy, a profession dedicated to alleviating human anguish, gained acceptance under a governance infamous in all history for institutionalizing brutality.

ROBERT G. L. WAITE
Williams College

WOLFGANG BENZ, editor. *Die Vertreibung der Deutschen aus dem Osten: Ursachen, Ereignisse, Folgen*. Frankfurt a/M.: Fischer Taschenbuch. 1985. Pp. 250.

The expulsion of the Germans from the East at the end of World War II is still a controversial subject. Feelings on the topic run high, and polemical works outnumber scholarly ones. This collection of sixteen essays treats the complex topic in a balanced and informative way. The postwar population transfer brought immense human sufferings, hardships, and individual injustices. The vast redistribution of population that took place at the time of the Third Reich's collapse became part of the staggering terror, savagery, and brutality of a Nazi regime bent on the destruction of entire races and nations.

The slim volume, edited by Wolfgang Benz, examines in a swiftly paced narrative the many-faceted dimensions of the expulsion, from its causes to its consequences, and succeeds in providing a comprehensive synthesis of the development of this population movement. The contributions adequately cover all the major themes. In his introductory remarks, Benz groups the narrative into three chronological parts. The first five contributions describe the circumstances and motives of the expulsion within the larger framework of post-1919 German policies. Norbert Krekeler brings out the nature of the Reich's German policy toward the German minority in Poland and shows that the policy begun in the Weimar Republic was continued by the Nazis. Rudolf Jaworski's superb essay stresses the all-German character of the Sudeten German nationality politics. Benz provides an insight into the process of Germanization in the occupied Eastern territories, arguing that the National Socialist policies were the direct cause of the postwar population transfer. In his analysis of the Allied policies up to the Potsdam Conference, Klaus-Dietmar Henke contends that the expulsion became an inevitable consequence of the power politics of the Allied powers. Josef Foschepoth's incisive essay explains the policy of the Western powers toward the Federal Republic.

The five essays in the second part are devoted to eye-witness reports and to the reprinting of two articles that were originally published in 1945 and 1947. When discussing a journalistic account by a Slovak historian who was strongly critical of the transfer of the Sudeten Germans, Eva Schmidt-Hartmann reveals her own bias and forgets that the historian's task has always been to strike a balance between various possibilities of interpretation. She shows little awareness that the war was not just another conflict but a deadly struggle against a brutal regime.

The excellent contributions by Franz J. Bauer, Falk Wiesemann, Reinhold Schillinger, Hermann Weiss, and Dietrich Strothmann in the book's final section deal with the postwar settlement and with the integration of the expellees into Germany, German policies toward the refugees, and the relative decline of their political influence in the Federal Republic of Germany. Hellmuth Auerbach concludes with an up-to-date bibliographical essay. The index is restricted to personal names. The two maps are fairly useful.

Inevitably, the scope of most essays is restricted. Factual investigation and not interpretation is the rule, and some readers may feel that the anthology treats the bare bones of history rather than its essence. With the exception of the uneven second part that does not seem to fit into the scholarly framework, the editor has done very well in integrating the diverse topics and varied literary styles and has produced a cohesive work without a single instance of overlapping material. Although aimed at the general public, the book is to be recommended to anyone interested in going beyond the superficial and polemical writing that has characterized the subject.

RADOMIR V. LUZA
Tulane University

CHARLES L. STINGER. *The Renaissance in Rome*. Bloomington: Indiana University Press. 1985. Pp. xv, 444. $37.50.

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During the last two decades, study of the Italian Renaissance, long concentrated on Florence, has been expanded first to Venice and, with recent works by John W. O'Malley, John F. D'Amico, and Melissa Bullard, to Rome. In this book Charles L. Stinger presents a lucid, remarkably learned, and more general account of the Renaissance in Rome. He demonstrates how, under the sponsorship of popes and other clerical patrons at the papal court during the late fifteenth and early sixteenth centuries, the artistic and philological achievements of the Renaissance were taken into the service of the church. While Renaissance art and architecture were making Rome into a fitting spiritual capital for the church universal, humanists were associating the universal authority claimed by the papacy with the universal empire of ancient Rome, in a process operating, as Stinger indicates, through "mood, myth, image, and symbol" and expressed in "liturgy, ceremony, festival, oratory, and art" (pp. xiii, xiv). In this way the new culture of the Renaissance, often associated elsewhere with ascending themes of authority, was assimilated into the ideology of papal monarchy.

Stinger sees this development as beginning with the return of Eugenius IV to Rome in 1443. It was continued by subsequent popes, notably, with Nicholas V's patronage of humanism and the militancy and imperial vision of Alexander VI and Julius II, and it reached an artistic climax under Leo X. It was ended, for Stinger—here perhaps too ready to accept a convention of Renaissance historiography—by the sack of Rome in 1527.

The Roman Renaissance was nourished by the general fascination of Renaissance culture with classical antiquity. Florentine humanists had begun to study the church fathers; in Rome, the fathers were seen as the link between the glories of the ancient past and contemporary Christianity. Ancient imperialism was also interpreted as pointing to papal Rome. The names taken by popes during this period—Pius, Alexander, Julius—suggested this. During the pontificate of Julius II a medal was struck bearing the inscription "Julius Caesar Pont. II." Support for papalist ideology was also discerned in the Old Testament; the pope was celebrated as uniting, like Moses, regal and imperial powers or was associated with Solomon, builder of the Temple.

The culture of Renaissance Rome superficially resembled Renaissance culture elsewhere in Italy, but Stinger shows that it was also profoundly different. The city was deliberately transformed "from medieval alley and tower to Renaissance street and palace" (p. 30) in order to deepen devotion to the Holy See on the part of crowds of pilgrims, especially in jubilee years when visits to Rome were rewarded with special indulgences. Humanism was no longer, as it had been in its origins, antagonistic to philosophy; in Rome it is hardly recognizable as a descendant of sophism. In spite of some emphasis in Rome on literal-historical exegesis of Scripture and the historicity of Saint Peter and local martyrdoms, Roman humanism rejected the historicism and cultural relativism implicit in the rhetorical tradition. Roman humanists explored Roman ruins not so much to reconstruct the ancient past as to unearth "talismans" of an eternal spiritual power. The more novel tendencies in Renaissance culture were suppressed and domesticated in Rome.

Stinger confesses, finally, that he finds the aggressive assertions of the Roman Renaissance grandiose and self-deluding, but he manages to treat his subject with remarkable understanding. He is especially sensitive in his interpretation of artistic evidence, and he has also worked through a mass of little-known literary texts. For a work of synthesis, his book is unusually close to its sources, and it is full of stimulating, sometimes elegantly phrased interpretation.

WILLIAM J. BOUWSMA
University of California, Berkeley

SAMUEL Y. EDGERTON, JR. *Pictures and Punishment: Art and Criminal Prosecution during the Florentine Renaissance.* Ithaca: Cornell University Press. 1985. Pp. 243. $32.50.

Samuel Y. Edgerton, Jr., explores the symbiotic relationship between the development of art and law from the thirteenth through the sixteenth centuries. Reflecting on aesthetic materials—some of which disappeared long ago and some of which art historians have largely ignored or considered "daubs"—Edgerton argues that "painters, sculptors, architects, judges and police could not have pursued their respective disciplines as they did without mutual interaction" (p. 13). This intriguing survey, sprinkled throughout with tantalizing and ghoulish detail, begins by analyzing paintings of the Last Judgment. In Florence and Siena as early as the twelfth century such paintings and frescoes, placed prominently in the chambers of communal justice, linked the realms of divine and earthly justice. Mathematical rules of perspective and early Renaissance empiricism closely structured the communal ideology of hierarchy and order.

From these "icons" Edgerton then investigates "one of the earliest models for state patronage of the arts," the *pitture infamanti*. The courts of the podestà and the captain of the people hired some of the most gifted Florentine artists—Andrea del Castagno, Botticelli, Andrea del Sarto—to aid the commune in punishing traitors and debtors who had been condemned in abstentia. These paintings were

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not a primitive form of voodoo but a punishment in themselves, a literal defamation of individual characters and family names. The efficacy of the defamation, Edgerton argues, depended on the artists' abilities to portray accurately the offenders whom the state thus sought to ridicule. This form of art and of jurisprudence survived longest in Florence, the cradle of Renaissance art. By the end of the Quattrocento it was beginning to disappear even there. Edgerton explains its end by the shift in aesthetics from Trecento Giottoesque and Quattrocento Massacciesque realism to the mannerist *bellezza* of the mid-Cinquecento.

After examining this art form (which, unfortunately, survives only in seven drawings by Andrea del Sarto), Edgerton turns to a discussion of public execution and a collection of *Tavolette* used by religious confraternities to guide the condemned to their executions. Edgerton argues that, despite the mediocre artistic value of these collections of paintings framed by wooden handles, they were fundamental in "raising the consciousness" of northern Italians and in preparing their reception of the humane values expressed in the Enlightenment's condemnation of torture and capital punishment.

In a text rich with illustration, from mannerist ecstasy to defecating devils and ghoulish descriptions of execution, torture, and the dissection of prisoners placed at the service of anatomically interested artists, the overarching arguments of this book are not always so satisfying or tantalizing as the building blocks of its narration. For instance, if a change in aesthetics—the move from "visceral realism" to mannerist elegance or "art for art's sake"—was the cause of the demise of *pitture infamanti*, Florence should have been the first place, not the last, to witness the decline of this particular marriage between the artist and the judicial system. Second, Edgerton fails to explain adequately the contrary relationship between the rise of more harsh and gruesome execution in the period from Lorenzo il Magnifico to Cosimo I and the supposed softening in the sensibilities of artists and, by extension, "bourgeois" society toward the suffering of criminals during the same period. Third, the claim that "the revolutionary realism of Florentine art . . . helped raise people's consciousness concerning the inhuman brutality of legalized torture and public execution" is unconvincing (p. 14). It appears, moreover, to contradict a central proposition of the book: that the transition from early Renaissance naturalism to the soft touch of mannerist spirituality marked the decisive turning point away from the gruesome relishing of the horrors of hell and the pains of the tortured. Edgerton, nonetheless, succeeds in bringing together questions of jurisprudence, society, and art during the Renaissance. If nothing else, we are left with a new way of viewing old and esteemed works of art (from Giotto's *Last Judgment* to Fra Angelico's *Lamentation* to Michelangelo's *Last Judgment*) as well as a new angle for understanding the old transition from late medieval "natural empiricism" to High Renaissance mannerism.

SAMUEL KLINE COHN, JR.
Brandeis University

AMELIO TAGLIAFERRI, editor. *I ceti dirigenti in Italia in età moderna e contemporanea*. (Convegno Cividale del Friuli, 1983; Serie Monografica di Storia Moderna e Contemporanea, number 8.) Udine: Del Bianco, for the Istituto di Storia dell'Università di Udine. 1984. Pp. 539. L. 12,000.

The unifying theme of this collection of papers from a conference held in Cividale del Friuli in the fall of 1983 has long been a source of intellectual interest on the Italian peninsula. Indeed, from Machiavelli to Pareto, Mosca, and Gramsci, some of the country's finest minds have explored the shortcomings and problems of Italy's *ceti dirigenti*, or ruling classes. Such interest is certainly understandable in light of the major role attributed to these groups in the political fragmentation of the Renaissance, the economic decline of the seventeenth century, the limits of the Risorgimento, and the collapse of the Liberal state. With papers that range from the early sixteenth century to the period proceding World War I, the participants at the Friuli conference attempted to address aspects of most of these great crises and dilemmas in Italian history. Although the majority of papers focus on local developments in the Veneto region of northeastern Italy, they are supplemented by studies on the south and other regions of northern and central Italy.

The book opens with three general introductory essays that cover the early modern period (sixteenth and seventeenth centuries), the period from the early eighteenth century to 1861, and the decades from unification to World War I. Giovanni Borelli's paper on the early period is the shortest but the most interesting. Borelli rejects the old view that Italy's ruling elites suffered a process of involution and moral exhaustion between 1500 and 1700. Rather, he sees the resurgence of the nobility and the consolidation of the patriciates as part of a subtle process of adaption to a particular set of circumstances imposed on the peninsula in these centuries. Tomaso Fanfani's essay on the century preceding unification reaffirms the standard interpretation concerning the consolidation of a national ruling class through a gradual process of fusion between old and new, conservatives and reformers, aristocrats and bourgeois. In a somewhat unfocused piece, Giovanni Zalin provides a survey and partial social profile of the parliamentary blocs, interest

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group associations, cultural circles, and the press that influenced the formation of economic policy in the five decades preceding World War I. None of the three essays provides any systematic discussion of theoretical issues inherent in the concept *ceti dirigenti*. Taken together, the authors use the term broadly, including not only the groups that controlled the state and organized economic and social life but also opinion makers as well as proponents of new ideas that subsequently became influential.

The body of the book consists of twenty-six papers that more or less fit into the chronological divisions indicated in the introductory essays. As one might expect, the papers vary enormously in their themes, scope, and overall quality. Clearly, the essays on the early modern era predominate both numerically (fourteen) and in terms of the high level of their research and analysis. It is difficult to do justice to the range and richness of these papers. Some of the issues addressed include heresy and aristocratic unrest, charity and social control, law and patricians, education, and the various mechanisms of assimilation and adaption pursued by local nobilities to survive the difficulties of the sixteenth and seventeenth centuries. Although most of the papers deal with conditions in the Veneto, two essays explore the persistence of aristocratic power in Sicily and the mainland south. Perhaps the most ambitious and challenging essay is Giovanni Stumpo's comparative study of the Florentine patriciate and the nobility of Piedmont in the seventeenth century. Stumpo demonstrates convincingly how the Piedmontese nobility was rejuvenated through the inclusion of large numbers of new men in the course of the century. Conversely, the Florentine patriciate remained a closed social group composed of a small number of families that declined slowly but steadily during the early modern period.

Piero Del Negro's study of the distribution of power within the Venetian patriciate stands out among the five essays on the eighteenth- and early nineteenth-century ruling elites. Opening with an informative historiographical discussion of his subject, Del Negro then analyzes demographic trends and the changing social composition within key institutions of power in order to shed light on the patterns of patrician stability and mobility. His findings challenge the standard interpretation about the immobility and stagnation of the Venetian elite, for he sees a significant restructuring within the patriciate that permitted the political system to survive without undermining traditional values.

On the whole, the essays on the era from unification to the First World War are the least satisfactory and reflect the relative paucity of work that has been done in this area. Of the six essays, four are limited biographical sketches of varying quality, while Giuliano Muzzioli's piece on the relationship between banking and agriculture is strictly an economic history that does not deal with the issue of the *ceti dirigenti*. With the exception of Giovanni Panjek's intelligent exploration of the interaction between political and economic interests during the protectionist campaign of the 1880s in Friuli, the reader interested in the latest research on nineteenth-century Italian elites would be better advised to consult the recent issue of *Quaderni Storici* (August 1984) devoted to the urban bourgeoisie and, in particular, to the fascinating work of Alberto M. Banti on patrimonial dynamics in Lucca.

It is difficult, if not impossible, to draw any sweeping conclusions about such an eclectic collection of essays covering a period of four centuries. Although these essays originated as conference papers, a surprising number of them are distinguished by their ambitious scope and the breadth of the issues they cover. If they do not advance any dramatic new interpretations, they do undermine any facile generalizations about the ruling classes and force us to appreciate the tenacity of the Italian elites as well as the rich variety and complexity of their responses to changing developments on the peninsula and in Europe.

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CARLA RUSSO. *Chiesa e comunità nella diocesi di Napoli tra cinque e settecento*. Naples: Guida. 1984. Pp. 491. L. 40,000.

For over a dozen years Carla Russo's work has been devoted to the religious history of southern Italy, especially the Neapolitan area. It has included an examination of seventeenth-century convents there (1970), the monograph *Society, Church, and Religious Life in the Old Regime* (1976), and essays on topics ranging from popular religiosity in the early modern period (1979) to an analysis of the methodology of French historians and sociologists of religion, by whom she is clearly influenced (1972). Her most recent work, *Chiesa e comunità*, continues her studies of specific parishes and their organization begun with the previous publication of chapters 3 and 4 in essay form.

Contrary to what one might expect from its title, the book deals not with the city of Naples but with its hinterland, which until now has received scant attention. This extraurban part of the Neapolitan diocese was divided into three administrative areas comprising, in all, thirty-five parishes in twenty-nine places, from small hamlets to sizeable towns like Torre del Greco and Afragola. The territory is first described; then we are shown its organization into parishes, which remained essentially unchanged

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from the sixteenth to the eighteenth century. The third chapter deals with incomes of the parish clergy, the fourth with the often-difficult relations between the clergy, the vestries responsible for the temporal goods of the parish, and the wider communities. The remaining three chapters examine centers of devotion other than parish churches (like chapels or confraternities), mechanisms for the religious instruction of the people, and outward manifestations of popular devotion. There is neither an index nor a bibliography.

A prodigious amount of archival material has been examined by the author, who used mainly records of episcopal visitations but also reports of missionaries (especially Jesuits), parish priests, parish accounts, and confraternity records. Although visitation records are particularly valuable because of their attention to detail, Russo rarely supplements them with other kinds of evidence drawn from nonecclesiastical sources. As a result, her work is curiously one-dimensional, lacking life, as it were, and failing to give the reader an insight into the sort of society in which the parish structures, so painstakingly described, actually existed. To do the author justice, she makes the parameters of her study clear at the outset; nevertheless, it remains an immense mass of material of interest only to the specialist.

Historians will find chapters 4, 6, and 7 the most valuable. Some light is shed on little-known lay boards (*fabbricerie*) in charge of the day-to-day running of parishes. One wishes for a clearer discussion of the social standing their members had in their communities; they were able to lend parish money to each other and to relatives, and their position must thus have been perceived as one of power. By contrast, the discussion of catechetical efforts after the Council of Trent is sufficiently full to give us yet another example of mass ignorance of Christian doctrine despite the efforts of church authorities. One surprise is the understanding attitude of several archbishops who encouraged penitents to confess to priests from outside the parish rather than force them to confide in their parish priests against their will. Russo provides a good discussion of the gradual decrease in the number of feast days and their rationalization in the eighteenth century as well as of organized popular devotions like processions in connection with Marian holidays, the Eucharist, and feast days of saints. Pilgrimages were conspicuously absent, since in the territory studied there was no shrine to attract them.

Russo cites only occasional examples of popular behavior, such as the custom of intimacy before marriage or the problem of incest, that worried ecclesiastical authorities. Such instances illustrating the difficulties of changing established social and familial patterns after Trent might have been profitably compared with similar difficulties faced by the clergy elsewhere.

Ultimately, this vast mass of valuable material lacks the "so what" part. One wonders why the author has stayed away from a final analytical chapter that absolutely demands to be written. She is a fine scholar whose *obiter dicta* are suggestive, but she did not follow the example of the French models she admires and give us a conclusion in the style of Gabriel Le Bras or Jean Delumeau, which is a real loss.

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TRACY H. KOON. *Believe, Obey, Fight: Political Socialization of Youth in Fascist Italy, 1922–1943*. Chapel Hill: University of North Carolina Press. 1985. Pp. xxi, 343. $29.95.

This book deals with one of the central issues in the history of the Fascist regime, the problem of the young. From its inception, fascism portrayed itself as a youth movement. The regime boasted that it had brought the wartime generation to power in 1922. Later, when fascism seemed to be mired in compromise with the established old order, Mussolini placed his hopes on yet another generation that would be educated in the Fascist schools after 1930. Tracy H. Koon quite ably analyzes the propaganda that was directed at the young and traces the development of the organizations that were established to accomplish the goal of youth mobilization. Throughout, she seeks to assess the success of these policies.

The major contribution of this study is its careful reconstruction of the institutional framework that was designed to incorporate students from the earliest age into a wide range of activities from sports to political indoctrination. Particularly valuable is Koon's analysis of primary school texts introduced after 1929. Her conclusion that many of these texts were quite well done and must have been generally effective is surprising. She qualifies this judgment, however, by noting that the impact of the official texts seemed to decline as the students became older. Moreover, within the schools at least, Fascist values were grafted onto older religious and social values that muted their significance. This compromise with the old order stemmed in part from the Gentile reform, which, ironically, was the first effort by the regime to reorganize the schools. This reform was so permeated by conservative and elitist values, however, that its provisions were often at odds with the goals of later fascism. To overcome its weakness in the university, the regime resorted to special faculties and courses on Fascist culture, all with mixed results.

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Koon is correctly concerned with the effect of Fascist propaganda on both male and female youth, although it seems that the conservative prejudices of the regime tended to limit the mobilization of young women. She offers persuasive statistical evidence that the regime had less success in reaching the young after they left school. This, coupled with the early age at which many ended their education, remained a constant curb on the totalitarian aspirations of the regime. Likewise, she also shows that mobilization of the young varied widely by geography as well as by sex and age. Participation in youth groups dropped off in the center and south and again in areas where strong Catholic Action organizations existed. Thus, Koon adds further evidence for the development of a strong Catholic alternative culture during the 1930s.

Her research also contributes to the vexing question of the degree of consensus that developed around the Fascist government after 1929. She notes that support for the regime began to decline after 1938 and supplies some evidence to bolster this point, but she might have addressed the problem more fully. The final chapter, which deals with the growing antifascism among young intellectuals, could give a distorted impression of resistance to the regime among the mass of young people. But this is a minor problem. As it stands, Koon's book is a smoothly written and well-researched study on a central issue in the history of fascism.

ALEXANDER DE GRAND
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TOIVO NYGÅRD. *Irtolaisuus ja sen kontrolli 1800–Luvun alun suomessa* [Vagrancy and Its Control in Finland in the Beginning of the Nineteenth Century]. Summary in English. (Studia Historica Jyväskyläensia, number 31.) Jyväskylä: Jyväskylän Yliopisto. 1985. Pp. 162.

Toivo Nygård has previously studied phenomena related to Finnish nationalism, but with the present study he broadens his interests. In this study of vagrancy and its control in early nineteenth-century Finland, he hopes to clarify social attitudes and mechanisms of social control for deviants.

The Russo-Swedish War of 1808–09 and the Russian conquest of Finland brought destruction, scarcity, hunger, disease, and general demoralization. The dislocation accompanying the war contributed to the problem of vagrancy; suddenly, former soldiers were unemployed and homeless. The rapid spread of venereal disease was another result of the war associated with vagrants. When the aggravating causes linked to the war gradually lessened with passage of time, other factors started increasingly to contribute to the problem of vagrancy. Population growth created a relative overpopulation because of the inability of the agrarian socioeconomic structure to absorb the vagrants and provide adequate employment for them.

Nygård defines his concepts and problems clearly and discusses his sources in some detail. After surveying the socioeconomic background, he discusses the judicial and administrative setup and attitudes concerning the treatment of vagrants. He provides a great deal of detailed statistical information, much of which is useful, but some of which does not contribute to his argument and seems rather meaningless. The study would have benefited from a more selective use not only of statistics but also of information and details of hearings and sentencing for vagrancy. Documents from hearings, in fact, seem to have been the principal source material for this work. Since the vagrants formed a significant element of the prison populations and patients in hospitals and clinics that treated venereal diseases, Nygård further discusses these groups in some detail.

Nygård's book complements some earlier studies on poverty and criminality in nineteenth-century Finland. He does so not by challenging the validity of the earlier research but by bringing forth a minor and marginal group in the nineteenth-century Finnish society that has not previously attracted such attention. As a result of his study, we know much more about vagrants and their treatment and also more about Finnish society as a whole in this period.

PEKKA KALEVI HAMALAINEN
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PERTTI LUNTINEN. *Sotilas-miljoonat* [The Military Millions]. Summary in English. (Historiallisia Tutkimuksia, number 125.) Helsinki: Suomen Historiallinen. 1984. Pp. 186.

When Finland became a grand duchy of the Russian emperor in 1809, Russian troops were stationed in Finland, which was close to St. Petersburg. A tiny Finnish army, entirely volunteer and separate, coexisted with the Russian troops until 1878, when conscription was introduced into Finland. Conscription somewhat increased the size of the Finnish army, but a much smaller percentage of Finns was conscripted than of Russians. The Finnish army was still independent of the imperial army, and Finnish soldiers could not be sent outside Finland. Eventually, imperial ministers of war pushed to integrate Finnish conscripts into the imperial army. The Finnish estates refused, and in retaliation the Finnish army was abolished in 1899. Finns were now to be conscripted directly into the imperial army. Rather than serve in what they considered a foreign army, many young Finns emigrated, mostly to North

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America, and older Finns refused to implement the new form of conscription. Faced with the prospect of arming and training possibly disloyal troops, the imperial government put its desire for Finnish conscripts on a back burner and shifted its immediate goal to getting the Finns to contribute to financing the imperial army.

The conflict over "the military millions" is the subject of Pertti Luntinen's interesting and innovative book, although he sometimes expands, generally helpfully, his discussion to other Russo-Finnish military matters less directly concerned with this conflict. The main lines of his account are simple: the Russians regularly presented the Finns with substantial (but hardly overwhelming) bills, and the Finns regularly tried to avoid paying any, let alone all, of them. The conclusion of Luntinen's account is, entirely appropriately, expressed in a short table: the Russians collected much less than they expected, and much of what they did collect was for such capital investments in Finland as bridges and railways of strategic importance, rather than for "the military millions" in lieu of Finnish conscripts. Given the disparity between Russian and Finnish power, this outcome was hardly predictable when the bills first began coming to Helsinki.

Luntinen's central scholarly contribution is to explain how, and to what degree, the Finns avoided paying these bills. The wheels of the imperial bureaucracy moved slowly, and that bureaucracy was not always united in its response to Finnish stalling. Ministers of war were more narrowly principled about the integrity of the imperial decision-making process than were ministers of finance, who were eager even for finnmarks appropriated by a democratically elected Finnish parliament, which was one of the rewards for Finnish participation in the Revolution of 1905. Confiscation was difficult because the Finnish cabinet had prudently deposited many of its assets in Western European banks. Even when the personnel of that cabinet was eventually Russified, in response to Finnish recalcitrance, the new personnel tried to keep Finnish revenues in Finland for use in governing Finland. What the imperial army did collect came, for instance, out of the educational and medical portions of the Finnish budget. This undeniable loss to the Finnish people may have been at least matched by the fact that, when 1914 came, no Finnish conscripts would die in the Great War. Luntinen does not enter this into his balance sheet, but he does a fine job of demonstrating that, even at the height of Russification, Finnish autonomy was more real than most previous scholars have claimed. This book is a model of careful, detailed scholarship not intended to move beyond the evidence, which is beautifully presented.

MARVIN RINTALA
Boston College

EDGAR ANDERSON. *Latvijas vēsture, 1920–1940.* Volume 2, *Ārpolītika* [History of Latvia, 1920–40. Vol. 2, Foreign Affairs]. Summary in English. Stockholm: Daugava. 1984. Pp. 697.

Volume 1 of this work described the diplomacy of independent Latvia to 1938. The present volume continues the story, with roughly the same cast of characters, to the end of 1940, by which time Latvia (as well as Estonia and Lithuania) had been incorporated into the Soviet Union. Edgar Anderson sees the climactic events of these three years as the result of a combination of factors: German and Soviet machinations, over which the Baltic states had no control; the seeming paralysis and indifference of the Western powers; and the weaknesses of the Baltic states themselves, which grew out of their geopolitical position and their failure to achieve any kind of effective cooperation. Lithuania continued its disputes with Poland, and Latvia and Estonia avoided entangling alliances with Lithuania for that reason. "In the entire period of independence . . . the heads of state of Estonia and Latvia exchanged visits only twice, but the head of Lithuania did not once visit Latvia or Estonia, and the heads of these states did not visit Lithuania" (p. 533). As the international situation worsened, each state's continued reiteration of neutrality and apparent belief in the efficacy of the League of Nations seemed the height of accomplishment. But the neutrality of small states at the end of the 1930s was something of a fool's paradise, and Anderson masterfully draws the contrast between the high-minded and reassuring prose of international agreements and the behind-the-scenes power plays. The geopolitical weakness of the Baltic states might—just might—have been overridden in the short run by military collaboration. Anderson suggests, in discussing the Soviet invasion of Finland, that "the combined armed forces of the Baltic states, under normal mobilization, would have been twice as large as Finland's and the frontier needing defense only one fourth as long" (p. 375). The single-mindedness of the Soviet Union in playing the hand dealt to it by the secret clauses of the Nazi-Soviet Pact, however, makes such speculation somewhat pointless.

A good third of this informative volume describes, almost day by day, the steps of the Soviet takeover. To outside onlookers, the process could well have seemed effortless. Appearances were helped, in Anderson's view, by the refusal of President Kārlis Ulmanis to flee when this was still possible in early June of 1940. Ulmanis had established his authoritarian regime on May 15–16, 1934, and had since that time governed without the *Saeima* (parliament). By staying in his post through the crucial weeks of mid-1940 and, indeed, until the Soviets had installed their puppet government,

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Ulmanis, in the author's eyes, "was forced to sanction with [his] signature, or at least with [his] continuing presence in the presidential palace, the destruction of the independent republic and the creation of the Soviet republic" (p. 424). A similar role was played by President Konstantin Päts in Estonia. Ulmanis was transported to the Caucasus a month later, where he died in 1942. In the meantime, on August 5, 1940, Latvia was "admitted" to the Soviet Union as the fifteenth republic. The incorporation concluded what in Anderson's view other countries saw as foreordained: "The military-political situation in the Baltic states and their authoritarian internal arrangements were decisive negative factors in the attitudes of other countries toward them. Others looked on the Baltic states as countries fated to disappear. The only question was, who would seize them?" (p. 537).

Volume 2 contains a detailed bibliography of sources used in both volumes of this work, an appendix of important diplomatic documents, and a twelve-page English-language summary. Both volumes together now comprise the best treatment of the subject in any language.

ANDREJS PLAKANS
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JACEK SOBCZAK. *Położenie prawne ludności tatarskiej w Wielkim Księstwie Litewskim* [The Legal Situation of the Tatar Population in the Grand Duchy of Lithuania]. Summary in English. (Prace Komisji Historycznej, number 38.) Warsaw: Państwowe Wydawnistwo Naukowe. 1984. Pp. 133. 120 Zł.

The settlement of Tatars in the Grand Duchy of Lithuania dates back to the fourteenth century and consisted at first of prisoners of war from the Golden Horde and the khanates. Later, during the Jagiellonian era of the fifteenth and sixteenth centuries, the disintegration of the Golden Horde and rampant conflict among the khanates led to voluntary emigrations to Lithuania. Such emigration lasted until the partitions of the late eighteenth century. The grand duchy's Tatar population was relatively small and probably never exceeded eleven thousand people. It was concentrated in the central provinces of Troki, Vilno, and Nowogródek and constituted one of the duchy's ethnic and religious minorities.

This minority forms the subject of Jacek Sobczak's monograph, which is based on a variety of Polish archival sources and an extensive bibliography of primary and secondary works. Preceded by an illuminating bibliographic essay, Sobczak's monograph basically consists of three lengthy chapters. The first outlines the history of the Tatar settlements. The second describes the various forms of Tatar organization—political, military, social, and religious. The third chapter deals with their legal rights and obligations, both public and private.

Among the interesting issues Sobczak raises is whether the Tatars were persecuted for professing the Islamic creed. Aside from several aberrations (the destruction of a mosque in Troki in 1611 and the vocal demands by a Vilno clergyman, under the pseudonym of Piotr Czyżewski, that the Tatars be deprived of their religious liberties), the answer appears to be in the negative.

Sobczak explores at some length a thornier and more controversial issue, which concerns the extent of Tatar political rights and, in particular, whether the "hospodar Tatars" (that is, landowners) were considered part of the nobility. Sobczak disagrees with a number of authors on this point. He argues that, although the Tatars possessed a social hierarchy complete with an elaborate terminology of titles and although military service was demanded of landholding Tatars (a characteristic obligation of the nobility in the grand duchy), the Tatars were deprived of the corresponding right (possessed by noblemen) of participation in the district sejmiks. The legal position of the Tatars is reminiscent of that of the Jews, another ethnic minority that was basically under the protection of the monarch.

Sobczak's monograph is a fine work of scholarship that casts light on one of the lesser-known peoples who inhabited the Lithuanian grand duchy.

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MICHAEL G. MÜLLER. *Die Teilungen Polens: 1772, 1793, 1795*. Munich: C. H. Beck. 1984. Pp. 133. DM 29.50.

The main contention of this work is that the partitions of Poland were epoch-making events in modern European history. They influenced the structure of the whole continent and, above all, the history of Prussia and Germany as well as determined the future relationship between the partitioning powers—Prussia, Russia, and Austria—that became bound together in power politics by the continuous necessity of defending the postpartitions status quo.

According to Michael G. Müller, even the two world wars, and the period between, reveal consequences of Poland's partitions. This is not only a revisionist study of the disappearance of Poland from the map of Europe but also a thorough survey of all the related materials, considered until now by scholars in the West only in dispersed analytical studies.

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The author does not refer directly to the two known syntheses explaining the fall of the Old Polish *Rzeczpospolita* that were provided by the Cracow school of historical thought and the Warsaw school of history (with Henryk Mościcki, Oskar Halecki, and Oswald Balzer from Lwów). But his contentions are related to both previous major interpretations of the partitions. He added other findings pertinent to the partitions, particularly to their international causes and to the power politics of the three partitioning states.

According to Müller, the partitions did not result from Polish shortcomings and faults—constitutional, economic, social, and political. He places the event against a broader East Central European and general European background. He assesses the changes in Russian power after the Great Northern War and the creation of the Russian empire by Peter the Great. He also analyzes Russian attempts to establish a protectorate over the Polish-Lithuanian Commonwealth before the partitions.

The first partition unleashed forces that predetermined further partitions and effected the disappearance of the state, with grave consequences for Poland, for Prussia and Germany, for the Hohenzollerns, and for all Europe.

The author favorably evaluates the Polish constitution of May 3, 1791, and examines the Kosciuszko insurrection. He perceives the history of Poland as emphasizing a concern for the society versus the state. Before the partitions, however, the Polish state was an institution capable of satisfying the needs of the people and was open to reforms required for the transition from the old feudalistic *Rzeczpospolita* to a modern form of society.

The author, concise and concentrating on his topic, inspires trust in his well-balanced judgments and his knowledge of the history of Poland, Prussia, Germany, and Europe. This is shown in the narrative as well as in his wide multilingual references with relevant comments and cross references. They occupy 30 pages of the book in addition to 3 pages surveying pertinent primary sources. The book of 133 pages includes, in addition, a 10-page bibliography, largely of the most recent publications, embracing a thorough consideration of new Polish works, one map of the three partitions of Poland, and an index. The author has previously published other studies on Poland and related subjects.

This work adds much not only to the historiography of Poland in Western languages but also to the history of the partitioning powers, particularly of Prussia and Germany, and to the history and understanding of Europe. It must be considered in teaching modern European history and in new textbooks on this subject, since earlier textbooks are obviously deficient on many points articulated by Müller with remarkable lucidity and mastery of sources. It has also contributed essential propositions to the perception of the historiography and to the development of historical processes from the seventeenth to the twentieth century in Central Europe and in the entire continent, enriched as it is by thought-provoking observations.

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ADAM WANDRUSZKA and PETER URBANITSCH, editors. *Die Habsburgermonarchie 1848–1918*. Volume 4, *Die Konfessionen*. Vienna: Österreichische Akademie der Wissenschaften. 1985. Pp. xvi, 864. S 896.

The appearance of the multivolume series *Die Habsburgermonarchie 1848–1918* is a landmark not only in the historiography but also in the present-day intellectual and cultural life of East Central Europe. When completed, the series will provide a detailed and systematic guide to all the main aspects of the common life of the peoples of the Habsburg monarchy during the last century of its existence: institutions, society, and economy, as well as social, national, and religious forces. But under the guidance of the late Hugo Hantsch, and more recently of Adam Wandruszka and Peter Urbanitsch, the series has evolved into something more than an indispensible handbook and starting point for further research; its appearance is also an act of scholarly statesmanship. Historians of all the various nationalities and successor states of the old monarchy are contributing to the series, which marks, therefore, the conclusion of a "peace treaty" among them. For all their persisting national and ideological differences, they are moving into a new era, taking as their task the maintenance of scholarly collaboration and cultural unity across political frontiers.

The present volume, *Die Konfessionen*, covers all the main state-recognized religions of the Habsburg monarchy, with the exception of a few small sects and splinter groups that have already been dealt with in the earlier volumes on the peoples of the monarchy. The core of the book consists of six essays: three on the Roman Catholic Church in Cisleithanian Austria (Peter Leisching), in Hungary (Moritz Csáky), and in Croatia (Ivan Vitezić) and one each on the Oriental churches (Catholic and Orthodox [Emanuel Turczynski]), the Protestant churches (Friedrich Gottas), and the Jews (Wolfgang Häusler).

None of these authors has an easy task. Given the structure of the Habsburg monarchy, each of these neat, unified, and seemingly obvious divisions in fact includes a great variety of heterogeneous material. Turczynski's essay, for example, has to cover two

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rival churches, each divided into three or four semi-independent national and regional groupings, scattered through all of the three main political units of the monarchy: Austria, Hungary, and Bosnia-Hercegovina.

For the most part, the authors accomplish their task by concentrating on two themes that adapt well to a loose framework of description and analysis. The first is that of the organization and administration of religion. This includes not only the familiar struggle of the Roman Catholic Church with the evolving Habsburg state but much else besides: for example, important local questions of diocesan appointments and boundaries, the struggle of minority religions for recognition by the state, disputes over church order among Protestants, and the rivalry of Orthodox and Reformed among Jews. The second theme is that of the close connection between the organization and administration of religion on the one hand and the national struggle on the other. Again, this appears in a multitude of instances, from the politically centralizing implications of the Concordat of 1855 down to the obscure struggle of the Slovak Lutherans to prevent Magyar gerrymandering of church districts.

Both of these themes are familiar enough in themselves. But it takes a reading of these essays, with their excellent emphasis on local disputes as well as on central conflicts, to make one truly grasp how the needs of the state, the ambitions of the nationalities, and the rivalries within and among the various religious groups all formed part of one and the same complex struggle for power. In this area, the volume is authoritative, and its value is enhanced by the fact that all works cited by the various authors are gathered into a splendid bibliography of sixteen hundred items. It is to be hoped that this practice, which was not followed in the earlier volumes, will be adopted throughout the rest of the series.

But the method of treatment adopted in the essays also has disadvantages. The essays tend to turn into mere catalogues, summarizing the provisions of laws affecting religion, giving potted biographies of national leaders who were also priests or pastors, noting the foundation of new diocesan seminaries, and so on. More important, the contributors are inclined to neglect those themes that do not fit easily into the loose study's framework. The "inwardness" of religion, for example—the evolution of religious belief, ritual, and experience—is dealt with only to a limited extent, and principally where it led to palpable organizational changes or disputes such as the revival of Catholic missions and the conflict of Orthodox and Reform Judaism. But the essays on Catholicism, for example, touch only briefly on such important subjects as the decline of local religious traditions and the spread of the internationally standardized Marian or Sacred Heart cults. In addition, the social aspects of religion do not receive very much attention. There is very little information on trends in church attendance; the involvement of the churches in emigration is hardly discussed; and the essay on Protestantism in particular should have provided more detailed analysis of the social composition of the Protestant churches. (In the case of other religions, it is only fair to say that such analysis was rightly omitted, since it would have largely repeated the excellent treatments in the volumes on the peoples of the monarchy. Also, the essay by Ferdinand Hauptmann on the Muslims of Bosnia and Hercegovina is a notable exception to these criticisms.) To some extent, these omissions reflect the state of research. But the authors have certainly missed the opportunity presented by the publication of this authoritative handbook at least to lament the state of research and thereby to urge it forward.

In one important respect this volume differs from its predecessors: all but one of the contributors are Austrian scholars. (The exception is from West Germany. This is not, of course, to say that the authors are all ethnic Germans.) The reason for this, one supposes, is that it is hard to find leading specialists in the history of religion on the faculties of universities in socialist countries. No doubt future volumes will have the same international complement of authors as earlier ones, a practice that is so much in keeping with the spirit of reconciliation that informs the series. As praiseworthy as that spirit is, this reviewer, at least, cannot help viewing the whole enterprise with a certain nostalgia. With the publication of these volumes, so detailed and objective, not disguising differences but on the whole devoid of controversy, the old monarchy itself, with all the loyalties it inspired and the hatreds it aroused, finally passes into history.

GAVIN LEWIS
John Jay College
City University of New York

DRAGUTIN PAVLIČEVIĆ, editor. *Vojna Krajina: Povijesni pregled, historiografija, rasprave* [The Military Frontier: Historical Survey, Historiography, and Articles]. (Biblioteka Znanstvenih Radova.) Zagreb: Sveučilišna Naklada Liber for Centar za Povijesni Znanosti, Sveučilišta u Zagrebu. 1984. Pp. 476.

This volume examines an important and neglected aspect of Croatian history. It is a collection of articles that were originally presented as papers at a conference on the Military Frontier sponsored by the Institute of Croatian History of the University of Zagreb in 1981, in commemoration of the one hundredth anniversary of the reincorporation of

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the Croatian and Slavonian Military Frontier into the Kingdom of Croatia, Slavonia, and Dalmatia.

Papers from such a conference can be a dull and diffuse group of brief inquiries into obscure topics, but that is not the case here. The collective approach is probably the only practical one at present, for the definitive history of the Military Frontier is still to be written. The articles in this book open up new questions and approaches and will undoubtedly serve as a springboard for further research. The papers selected for publication from among the many presented at the conference represent the work of distinguished Yugoslav and foreign scholars. All of the contributions are based on original research and are richly documented. There are twenty-eight articles in all.

The volume opens with an important essay by Jaroslav Šidak, the dean of Croatian historiograhy, on the meaning of the Military Frontier in Croatian history. The rest of the articles are organized under five headings: "Historical Summary," which contains essays written specifically for this book by Fedor Moačanin and Mirko Valentić, both of whom have worked extensively on the history of the Military Frontier, and which provides a needed framework for the following sections; "Sources and Historiography of the Military Frontier"; "History of the Military Frontier from the Sixteenth Century to the End of the Eighteenth Century"; "The Military Frontier in the Nineteenth Century"; and "Aspects of the Culture of the Military Frontier."

The Military Frontier began in the sixteenth century as a series of fortifications in the Kingdom of Croatia (part of the Kingdom of Hungary) organized as defense against the advancing Turkish armies. On the death of Louis II of Hungary in 1526, Ferdinand of Austria was elected king of Hungary (1526) and king of Croatia (1527), and the burden of border defense against the Turks gradually shifted to the Habsburgs, first to Inner Austria, then to the emperor. As civilian inhabitants fled endangered areas, those who remained were joined by periodic waves of emigrants from Turkish-held territories, many of whom were Serbs. With the stabilization of the Habsburg-Turkish border in the seventeenth century, the organization of the Military Frontier took on a more permanent form and came increasingly under Habsburg control. This was vigorously contested by the Croatian government and feudal nobles, but the Habsburgs prevailed and the Military Frontier was finally separated from the other lands of Croatia and Slavonia.

Demand for the return of the Military Frontier became a constant theme in Croatian political history. The Military Frontier was a land of peasant soldiers who received grants of land in return for perpetual military service. They were ruled by their officers, who served as the local military authorities and government. In the eighteenth century the Military Frontier was expanded into areas of Croatia and Slavonia recently liberated from the Turks. Although some of the older frontier territories were no longer on the Turkish border, they remained within the Military Frontier. In the eighteenth and nineteenth centuries the Military Frontier served as a border force against a declining Turkish empire and as a cheap reserve of military power for the Habsburg empire. Trade and industry were poorly developed, farming was for subsistence, illiteracy was high. Disbandment of the Croatian and Slavonian Military Frontier began in the 1870s, and the region was finally rejoined to the rest of Croatia and Slavonia in 1881.

The articles examine the history of the Military Frontier from many sides: military organization, economic and social development, architecture, literature, ethnic composition, sources of discontent, spread of Croatian nationalism, and reasons for the original separation from the other Croatian territories and for reincorporation in 1881. The historiographical essays give an exhaustive analysis of literature on the topic, and several studies point up the particular nature of the Military Frontier by comparing it to similar regions in Hungary and Russia. Several themes surface throughout the book. The first is a clear awareness that many of the major questions have only begun to be examined. The second is the gradual decline in the status and living conditions of the peasant soldiers. They were a privileged group in the seventeenth century, when compared to the serfs in the other parts of Croatia, an exploited group by the nineteenth century. These articles probe the myth of the happy, naive *Grenzer* who sought only to serve his emperor.

This collection of articles is very important. It removes the study of the Military Frontier from the dusty confines of military and diplomatic history and examines its development as a particular historical region.

ELINOR MURRAY DESPALATOVIĆ
Connecticut College

FRED SINGLETON. *A Short History of the Yugoslav Peoples*. New York: Cambridge University Press. 1985. Pp. xiii, 309. Cloth $39.50, paper $12.95.

It is almost impossible to write a short history of the Yugoslav peoples, who have such diverse and complex histories before they were united into one state in 1918. Fred Singleton has made a brave attempt. Less than a century old, Yugoslavia is still in an early phase of national development, but, because of its important geographical position and its unique sociopolitical history as the first Communist state openly to oppose Moscow, it is of special interest to

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students who need a basic guide to begin studying its history.

This book, which covers an immensely wide field, provides such a guide. Five chapters describe geography, early settlement, the region's occupation by Ottoman Turks and the Habsburg empire, as well as movements for independence from foreign rule. Three chapters deal with the First World War, the foundation of the Yugoslav kingdom, and the stormy interwar period. One chapter gives a pithy, balanced, but inevitably sometimes controversial, account of events of the Second World War: it covers the occupation, Četniks and Partisans, resistance, and the emergence of Tito as the leader of the Communist postwar government.

The last five chapters cover transition to the postwar socialist state: the elimination of opposition, relations with churches, handling of important constitutional and economic problems, foreign affairs and relations with the Soviet Union. Two of these chapters briefly describe repeated attempts made to introduce a new and specifically Yugoslav design for a Communist state run on a self-management system. One chapter covers Tito's last years, and another looks at Yugoslavia without Tito. Singleton tries to stand aside from the controversy.

Inevitably, with such a broad canvas the many subjects cannot be analyzed in depth, and the book contains little original research but shows wide reading of secondary works. It is clearly and interestingly written, and for those who wish to follow up their special interests excellent bibliographies follow each chapter.

PHYLLIS AUTY
EMERITUS
University of London

PHILIP LONGWORTH. *Alexis: Tsar of All the Russias*. New York: Franklin Watts. 1984. Pp. xiii, 305. $18.95.

Philip Longworth has published herewith a superb biography of Alexis Michaelovich, ruler of Russia from 1645 to 1676. Nine unusually well written chapters trace Alexis's life and reign against the background of the Russian past. The concluding chapter assesses Alexis's philosophy and style of government; his church-state problems, policies, and innovations; his efforts at military reform; his relations with foreigners and their governments. The book is based on an impressive array of published sources and secondary accounts and extensive archival work in the USSR and elsewhere. Twenty-eight pages of footnotes, a select bibliography, two useful endpaper maps, ten illustrations (eleven if one counts the elegant dust jacket), and an unusually helpful index complete this fine work.

Longworth argues that Alexis has been underrated by contemporaries and many historians. In Longworth's view, Alexis began as "an intelligent young man who placed a high value on ideas." Experience refined and enriched his intellect, and to the end of his life the tsar pursued a "coherent philosophy" of government. Admitting that "the conventional view of Alexis" presents him as "a passive figure manipulated by his favorites," the author accounts for the development of this image while demonstrating—correctly, in my view—that Alexis was actually a thoroughly "effective autocrat" after the first three years of his reign. In the final, analytical chapter from which these quotations stem and in the body of his narrative, Longworth demonstrates that "when Alexis acted openly he acted decisively" whether the problem was "large or small." Indeed, Alexis did much to fashion the autocratic state that his more celebrated son, Peter the Great, developed still further. Longworth provides a generous but reasonable assessment of Alexis as a diplomat, military reformer, and commander in chief. We are shown how skillfully Alexis reconciled a personal appreciation of Western culture and a determination to borrow its learning with the need to placate Russian xenophobes. All in all, Longworth is mildly revisionist in offering us a more able and accomplished tsar than the figure presented by such historians as S. F. Platonov and V. I. Kliuchevskii.

Although this is the finest biography I know of the second Romanov tsar, it is not, as the publisher advertises, "the first full biography of Alexis in any language for 150 years," nor would everyone agree with Longworth's but slightly more modest claim that until this effort Alexis "lack[ed] a biography that does him justice" (p. 2). Indeed, in some respects Longworth does not do Alexis or his period justice. The few paragraphs on Alexis as an entrepreneur do not indicate the close, personal leadership Alexis gave to adapting foreign industry to Russian soil and to broadening his country's international trade in more than one direction. Although Longworth infuses a bit of life into Patriarch Nikon, he makes strikingly little effort to develop secondary characters. Such figures as B. I. Morozov, A. S. Matveev, and A. L. Ordin-Nashchokin remain mere names, and their lives are not focused in such a manner as to show the reader their significance. This is not a minor complaint when the main subject is Alexis, for he granted these and other favorites sufficient power and latitude to make them important figures in the history they made together. But these and other possible objections would not be made to deny the numerous virtues of this well-written, thoroughly researched, intelligently conceived life of tsar Alexis Michaelovich.

JOSEPH T. FUHRMANN
Murray State University

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G. A. NEKRASOV. *Vneshniaia torgovlia Rossii cherez Revel'skii port v 1721–1756 gg.* [The International Trade of Russia through the Port of Revel]. Summary in German. Moscow: Nauka. 1984. Pp. 271. 3 r.

G. A. Nekrasov, a well-known authority on Baltic history, introduces this detailed monograph with a survey of the role of all the Baltic ports in Russian foreign trade throughout most of the eighteenth century. The most useful part of the book, it offers the clearest and best-integrated summary in any language of the attempt of the Russian government to integrate the newly acquired Baltic lands into the economic life of the empire while maintaining close and profitable commercial relations with Western Europe. The state employed a variety of devices to accomplish its ends, including preserving the special privileges of the Baltic merchants, erecting a tariff barrier between the Baltic lands and the rest of the empire, and carefully calibrating export and import duties in order to satisfy the financial needs of the treasury on the one hand and to divert certain kinds of products to St. Petersburg, the favored port, on the other.

Nekrasov then turns to Revel itself, although it is not clear why he chooses to devote the rest of the book to what was, after all, only the third Baltic port of the empire after St. Petersburg and Riga. As Revel was economically integrated into the empire, it functioned as an "outer harbor" of St. Petersburg, a transit port serving the entire Russian market, and, as in the past, a regional center for the immediate hinterland. The author admits that Russian merchants did not compete with either non-Russian merchants operating within the empire or foreign merchants engaged in long-distance trade with Europe, but he remains silent on the social consequences of rivalry among different ethnic groups. He attributes the uneven growth of Baltic trade to external events, mainly the intrusion of Western fleets into the Baltic and war involving the riparian powers. His statistical evidence, based on a thorough examination and evaluation of a variety of sources including commercial accounts, provides a useful correction to the more pessimistic view of the economic integration of the Baltic lands. But the author agrees with most prerevolutionary specialists that the government's tariff policies were often determined by the financial needs of the treasury rather than by the class interests of the gentry—in this case, the German landlords. He also implies that strict regulation of trade by the central government subordinated the commercial prosperity of Revel to the general welfare of the empire as a whole. In sum, his judgment of the advantages that Russian annexation of the Baltic lands had for both regional and national interests is delicately balanced. Because his account is rather narrowly focused, however, it is still necessary to consult Ia. Ia. Zutis's *Politika tsarizma v Pribaltiki v pervoi polovine XVIII v.* (1937), which remains the standard treatment of the general problem.

ALFRED J. RIEBER
University of Pennsylvania

GARY MARKER. *Publishing, Printing, and the Origins of Intellectual Life in Russia, 1700–1800.* Princeton: Princeton University Press. 1985. Pp. xiii, 301. $30.00.

In this book Gary Marker delineates the history and nature of publishing and printing in eighteenth-century Russia. The work is divided into chapters on printing and the Petrine revolution, the church and the Academy of Sciences, schools and publishers, private publishing, publishing in the provinces, the Russian book trade, book sales and reading, and censorship. It is well written, thoroughly researched, and carefully documented.

The central theme of the study is the evolution, often in a thesis-antithesis manner, of publishing and printing from government monopoly under Peter I (preceded by 150 years of church control), to relative control by elite institutions and private groups. The government, in rather massive doses under Peter I and Catherine II, hoped to use the written word to create a new kind of citizenry—one that was literate, active economically, and, above all, loyal. But no monarch had the time or resources to incorporate the publishing and writing world fully into the campaign to create right-thinking citizens. For that reason, Russian governments eventually had to leave such matters to individual writers. But a watchful eye was always kept on them, and administrative mechanisms for secular censorship also evolved as the century progressed.

Although the book is focused mainly on publishing and printing matters, and herein lies its novelty and main contribution, the author also brings together most of the crosscurrents of Russian intellectual life in the eighteenth century. Among these are the consuming question of growth of Russian patriotism, usually in reaction to foreign intellectual domination, and the importance of the University of Moscow in this trend. Marker notes the significance of the appointment of M. M. Kheraskov as the overseer of Moscow University Press in 1757 and the consequence of this event for Moscow's burgeoning intellectual life. He also outlines the importance of textbook printing and the stimulus to the industry caused by various school reform projects, above all, Catherine's efforts in the 1780s.

The wide range of the study does lead to a lack of detail in some matters. For example, some might

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feel that G. F. Müller's *Monthly Works* and its book review section—Russia's first—could have been granted more attention. But this does not detract from the value of the book, which should be read by any student or scholar interested in the cultural life of eighteenth-century Russia.

J. L. BLACK
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MARIANNA TAX CHOLDIN. *A Fence around the Empire: Russian Censorship of Western Ideas under the Tsars.* (Duke Press Policy Studies.) Durham, N.C.: Duke University Press. 1985. Pp. xii, 281. $42.50.

Founded in 1828, the Foreign Censorship Committee served mainly to screen all publications imported into imperial Russia for bookstores and libraries. In part 1 of her book about the committee, Marianna Tax Choldin describes a shift from bureaucrat-censors under Nicholas I to poet-censors under Alexander II; in part 2 she particularizes forbidden ideas by systematically sampling German books that were "banned" from 1856 to 1896.

In her chapter of figures and tables for 1856–96, Choldin admits the "limitations of statistical samples." Her three line graphs and twelve tables, however, are further limited by inconsistencies and imprecision. As one example, her graph on banned publications (fig. 5.1) shows none for 1883 and 1885, but the one on the percentage of books banned (fig. 5.2) records 5 percent for both years. Moreover, Choldin confuses the issue by using the term "banned" to mean censored in any degree—totally, partially, or conditionally. She thus cites an "overall probability of banning of less than 10 percent" in 1856–96 (p. 128). To find out the probability of a work's being totally banned, the reader can infer from table 5.7 that the figure is less than 5 percent.

Although the political masters set the rules on acceptability for the censorship, Choldin concentrates on the men who did the enforcing, the censors. Many were petty, but the reader gets some surprises about others. Those tagged as poet-censors were a fraternity of honorable and "caring" defenders of good literature who made their committee meetings a weekly literary soirée. From M. L. Zlatkovskii, who is a primary source, we learn that the chairman, the poet Fedor Tiutchev, valued the committee highly for its role in admitting good foreign works into Russia. His colleagues and fellow poets Apollon Maikov and Ia. P. Polonskii shared his views. So, too, Zlatkovskii praises another colleague, E. Kh. Shul'ts, known as a "titan in his field," for lugging home great piles of books in assorted languages and returning a week later with written assessments of each one.

Day-to-day details like these take precedence over a systematic appraisal of policy within a historical context. Choldin has chosen a discursive approach, with frequent asides to parallels in other times and places (some distractingly loose, such as her likening of the imperial censors' excision of certain passages in a history of Judaism to the Soviets' excluding all mention of Jews in textbooks on ancient history [p. 184]).

This said, Choldin has used her skills as a librarian to good effect in categorizing by subject a broad sample of the banned publications and in discovering the names and identities of over one hundred censors. An annotated list of censors appears in an appendix.

CHARLES A. RUUD
University of Western Ontario

J. ARCH GETTY. *Origins of the Great Purges: The Soviet Communist Party Reconsidered, 1933–1938.* (Soviet and East European Studies.) New York: Cambridge University Press. 1985. Pp. viii, 276. $34.50.

This monograph, despite its intriguing title, appears at first glance to verge on redundancy. Western historians—and Roy Medvedev in the Soviet Union—have written at length on the Soviet Communist party and the Great Terror, and numerous biographers have dissected the life and times of Joseph Stalin. What more can possibly be said without access to Soviet archives? J. Arch Getty has not received that implausible dispensation, but with energy and persistence (and a reworking of previously available sources) he has fashioned a "revisionist" work that commands attention and respect. Whether he will also gain widespread approbation is doubtful, for, among other transgressions, he has violated the ground rules of a generation of Kremlinologists (or Sovietologists, to use a less invidious term). In short, he questions several tenets of Western orthodoxy—what irreverent younger scholars have labeled the "totalitarian model."

The author has not, however, posted a manifesto assailing the ideological strait jacket forged by his predecessors, nor has he sought to widen the debate along the provocative lines of Stephen F. Cohen's *Rethinking the Soviet Experience* (1985). And he avoids altogether such controversial issues as a "body count" of political victims. Nevertheless, he finds "untenable" the conventional assumption that Stalin's "diseased psyche" and other manifestations of his baleful rule were responsible for the reign of terror of the 1930s (p. 3). His evidence, much of it adduced in tedious detail, does appear to substantiate his contention that inefficiency and other

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nonpolitical offenses at the lower party level characterized the early purges of 1933–36 and that the penalties exacted were seldom more serious than expulsion. He also attacks with some success the accepted view that Stalin, as a kind of *deus ex machina*, carefully plotted every political initiative made during the purge era. But traditionalists may feel that even this limited "rehabilitation" of the celebrated tyrant strays beyond the sources. Probably the crucial test in any attempt to validate the Getty "thesis" is the Kirov affair. Sergei Kirov, the powerful boss of the Leningrad party organization, was murdered on December 1, 1934, by a lone assassin, and the master conspirator is thought to have been Stalin himself. The author disputes this version in a separate appendix. Although he scores a few debater's points, his case is not convincing, in part because he minimizes the testimony of Nikita Khrushchev, whose "secret speech" in 1956 tended to confirm earlier judgments and whose private remarks were even less discrete in linking Stalin to the crime. Other documentation, chiefly emigre memoirs, is rejected as unreliable, an argument pursued in a trenchant if not entirely persuasive bibliographical essay.

Compared to Robert Conquest's magisterial *The Great Terror* (1968), this book is erratic, opinionated, and unduly technical. Yet, to be fair, it makes no pretense to comprehensiveness, and an iconoclastic study that dares to probe a major subject is a refreshing departure from the norm of academic diffidence. It should be welcomed as a challenge to future scholars, although, admittedly, their task will not be easy.

ROBERT D. WARTH
University of Kentucky

V. V. ALEKSEEV *et al. Rabochii klass Sibiri v period uprocheniia i razvitiia sotsializma* [The Working Class of Siberia in the Period of the Strengthening and Development of Socialism]. (Istoriia Rabochego Klassa Sibiri.) Novosibirsk: Nauka. 1984. Pp. 373. 8 r. 50 k.

This book falls squarely within the well-established genre of collectively authored histories of the Soviet working class of particular cities, regions, and republics during certain canonical periods. As V. V. Alekseev, the head of this authorial collective, explains in the introduction, the period of the consolidation and development of socialism in the USSR began in 1938 with the Third Five-Year Plan, continued through the years of the Great Fatherland War (1941–45), and reached its conclusion in the course of the Fourth, Fifth, and Sixth Five-Year Plans (1946–60), after which Soviet socialism is supposed to have entered its developed or mature phase.

It is obvious that the authors themselves worked according to an elaborately detailed plan. Each of the book's three chronologically conceived sections begins with a discussion of economic developments and of changes in the size and composition of the Siberian working class, which is defined as manual industrial, transport, and state-farm workers. The authors then proceed to consider workers' skill levels and technical training, production activism (socialist competition and individual initiatives for improved technique), and participation in public, political, and administrative activity. Finally, they turn to the living conditions and cultural life of the region's workers. More than half the book (201 pages) concerns the postwar years, but, proportional to the length of each period, the years 1941–45 receive the most intensive treatment. new issues or advance new interpretations, and this one is no exception. Moreover, scant attention or no attention at all is given to matters that shaped the working class of Siberia but are still off-limits to Soviet historians. Thus, we are told that the main "subjective difficulty" in the development of Soviet democracy before the war was the cult of personality (p. 46), but we do not learn what the effect of the cult was. We are presented with tables listing, for example, how many tons of cement and millions of bricks were produced in each region of Siberia, but we can only guess whether forced labor was involved in their production. (Indeed, there is not a single reference in the entire book to the camps or their inmates.) The change in the composition of the working class after the war is discussed with reference to demobilization but not to the release of prisoners from the camps in the mid-1950s nor, except *en passant*, to the resexualization of occupations. Finally, scarcely any information is provided on the ethnic composition of the Siberian work force or on cultural facilities for those whose native language was not Russian.

What we get instead is decree history with statistics *ad nauseum* (but no maps) chronicling the regularity of development of Siberian industry and its work force. In short, while it may serve as a reference tool, this book should not be read without a good supply of No-Doze.

LEWIS H. SIEGELBAUM
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V. I. KLOKOV. *Deistviia partizan Ukrainy na zheleznodorozhnykh kommunikatsiiakh v tylu fashistskikh voisk, 1941–1944* [The Actions of the Ukrainian Partisans on Railroad Communications in the Rear of the Fascist Troops, 1941–44]. Kiev: Naukova Dumka. 1984. Pp. 166. 1 r. 50 k.

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In contrast to most of the numerous Soviet publications on guerrilla operations during World War II, V. I. Klokov's little book presents a monographic examination of one significant aspect rather than a narrative of heroic deeds designed to inspire Communist youth. Although the book unfortunately contains no index, it has copious footnotes to Soviet archival materials and publications. Many Western accounts are omitted, notably, those in English. Klokov does use important West German materials, including one publication by Hermann Teske (the Wehrmacht officer most concerned with keeping traffic moving), but not Teske's lengthiest work, *Der silbernen Spiegel: Generalstabsdienst unter der Lupe* (1952).

Klokov's book is also franker in admitting shortcomings than were earlier Soviet histories. He concedes the failure of the mid-1941 effort to organize partisans regionally, pointing out that only thirteen "underground *obkoms*" (party committees) reached their assigned stations. Although military men cut off behind German lines reinforced the scattered guerrillas, the improvised partisan units were rarely coordinated by higher headquarters until 1942. The first severance of a Ukrainian rail link (in the forested Chernigov region) was not accomplished until October of that year. Disruption of rail communications in the more important Ukrainian steppe regions depended, until many months later, on underground conspirators. In contrast to the coordinated destruction that partisan detachments sometimes achieved, early underground sabotage was necessarily sporadic and could not sustain serious disruptions of traffic.

Klokov rightly criticizes the dominant emphasis in Soviet partisan accounts on activities like attacks on occupation garrisons and establishment of partisan base areas and highly detailed discussions of party organizational procedures, although his own book provides a useful summary of major innovations in the chain of command. Certainly he is right in emphasizing that interruption of rail traffic was, potentially, the most significant partisan contribution to the Soviet war effort. He correctly highlights the Kursk operations (a counterstroke to the abortive German *Zitadell* offensive in the summer of 1943) as the single most important guerrilla attack. For a brief but significant time this partisan operation nearly paralyzed German transportation to the critically important front. Like other Soviet writers, though, Klokov exaggerates the overall military impact of the "war on the rails," notably, in comparison to Western Allied air bombardments—although by implication he also admits the inefficacy of Soviet aerial operations.

More significant is Klokov's virtual omission of what many outside analysts (including this reviewer) and some Soviet scholars have seen as the principal long-term function of the partisans: maintenance of a semblance of Soviet power over populations in areas under nominal German control. It would be unfair, however, to criticize a Soviet author too severely for declining to explore such a dangerous subject, especially when it is peripheral to his well-defined monographic theme.

JOHN A. ARMSTRONG
University of Wisconsin

A. IU. BORISOV. *SSSR i SShA: Soiuzniki v gody voiny, 1941–1945* [The USSR and the USA: Allies in the Years of War, 1941–45]. Moscow: Mezhdunarodnye Otnosheniia. 1983. Pp. 285. 1 r. 60 k.

The recent flood of Soviet publications on the Second World War reflects an implicit recognition that the war was the major legitimizing experience that aligned rulers and ruled against the external foe. This production of planned research also draws attention to a lesson, more directly relevant to this title, that the authorities are happy to stress: if the superpowers could cooperate in war, they can cooperate in peace as well.

At the same time, as A. Iu. Borisov's book illustrates, the treatment of Soviet-American relations during the war years is a subject full of pitfalls and (to use Soviet jargon) contradictions. For instance, FDR is at times portrayed as a good leader and a "realist" and at other times as a naive and unwitting tool of imperialist interests. If, as the author argues, the successful aspects of the wartime alliance resulted in substantial measure (on the American side) from Roosevelt's personal role, then it is hard to draw lessons for the future without positing the availability of other American leaders with comparable authority and a similar outlook on the USSR. The deterioration of Soviet relations with the Western allies is of course attributed entirely to the "imperialists," yet the book proudly proclaims that one major consequence of the war was "an unprecedented revolutionary upsurge throughout the world" that, "like a boomerang, struck the imperialism that had prepared and unleashed it" (p. 9). On the whole, relations between the Allies went well, and this is a lesson to remember; relations eventually deteriorated because the U.S. delayed opening the second front, evaded a variety of commitments, and sought to deprive the Soviet Union of a fair share of the spoils of the war.

Even American scholarly attempts to split the blame for failures and tensions in Soviet-American relations are dismissed. Isolationism was a cloak intended to conceal the monopolists' designs to redistribute power among the imperialists in favor of the U.S. We are told that it is a calumny to speak of Soviet expansion: whereas Western wartime pol-

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icy was "aggressive," the Soviet liberating mission abroad is a proud memory.

The book offers little that is new in terms of sources and information. For the Soviet reader there are some worthwhile (and some worthless) references to U.S. publications whose existence is not generally advertised in the Soviet Union. For the Western specialist there are a few references to one highly unrevealing file of the foreign ministry archives dealing with Soviet diplomatic reports on U.S. media and public opinion.

In its chronological accounts of major issues—such as the second front, lend-lease, the future of Poland and Germany, Teheran and Yalta—the book is rather well documented. It is essentially a familiar argument, thoroughly uncritical of Soviet behavior, and politically selective in what is and is not treated and cited. All in all, this is a curious but unimportant book, scarcely a landmark in the history of the war.

ALEXANDER DALLIN
Stanford University

NEAR EAST

KEMAL H. KARPAT. *Ottoman Population, 1830–1914: Demographic and Social Characteristics*. (Turkish and Ottoman Studies.) Madison: University of Wisconsin Press. 1985. Pp. xvi, 242. $50.00.

Historians have increasingly turned to demographic statistics in order to clarify and analyze the social, economic, and political transformation of societies. Data on the changing composition and characteristics of the population and on births, deaths, and migration drawn from official sources of statistics and government archives have provided new bases for comparative historical studies and for in-depth and detailed histories. Using a wide range of statistical sources, Kemal H. Karpat's impressive book on patterns of change in the Ottoman empire in the nineteenth and early twentieth centuries is one such example. Karpat painstakingly collects, carefully evalutes, and systematically analyzes a wide range of previously unavailable administrative records and census sources, and throughout he demonstrates a thorough awareness of research studies in the area.

The monograph contains well over one hundred pages of statistical tables and five chapters describing and analyzing the data in the context of detailed historical patterns. Karpat emphasizes the central role of migration in the transformation of the Ottoman empire. The analysis represents an important corrective to previous studies not based on statistical information and often biased by political interests. After presenting a conceptual and methodological overview of issues in the study of the Ottoman population, the author argues for the value of official sources and reviews the problems of changes in administrative divisions and issues of population undercounts. Demographers might quibble with the argument that the data can be corrected by adopting a simple "margin of error" percentage, and they would certainly prefer the application of more sophisticated population models to evaluate data deficiencies and correct for errors. Nevertheless, the data represent an invaluable source for further statistical adjustments and manipulations.

Karpat also surveys the religious and ethnic distribution of the Ottoman population, focusing in particular on Greeks, Bulgarians, Armenians, and Muslims. The clear documentation shows that immigration and the demographic transformation of the Ottoman state were accompanied by economic growth, social changes, and political mobilization. The analysis of population movements demonstrates how economic differentiation and social class formation in the nineteenth century were linked to cultural variation and ethnicity and how migrations altered the traditional social structure, paving the way toward political and social reorganization. The detailed examination of Istanbul highlights immigration, the development of economic capitalism, and the changing ethnic-religious heterogeneity. As a microcosm of change, Istanbul illustrates the intensity of population and social transformations and their interrelationships.

Students of Ottoman history will find the data and the analysis of great value for understanding Ottoman population and society in the nineteenth and early twentieth centuries. Demographic historians will learn much from this case study. We eagerly await further analysis and explorations of these valuable materials.

CALVIN GOLDSCHEIDER
Brown University

AMNON COHEN. *Jewish Life under Islam: Jerusalem in the Sixteenth Century*. Cambridge: Harvard University Press. 1984. Pp. xii, 267. $30.00.

Amnon Cohen's *Jewish Life under Islam* describes the life of the small Jewish community in Jerusalem, a provincial town of the Ottoman empire in the sixteenth century. With a population base averaging about twelve hundred souls over the century, this group at first glance is a narrow base for generalization. Yet Cohen's book fulfills its claims better than other recent publications with equally ambitious themes (for example, Bernard Lewis's *The Jews of Islam* [1984]). The difference lies in the sources used and the assumptions made. Whereas these other recent books depend heavily on secondary

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and literary sources and the assumption of an essentially unitary nature to a thousand years of the Islamic Middle East, Cohen rejected this easy course. He has, instead, worked his way through the mountain of primary documentation available to scholars of the sixteenth-century Middle East, the Jerusalem court records, and, making no assumptions at all, he has come up with the most accurate picture to date of Jewish life in that time and place—and perhaps for other places as well.

Cohen first discusses the relationship of the Jerusalem community to Ottoman governmental authorities. His study clearly documents that the Jewish communities had no central imperial administrative hierarchy, no Jewish millet paralleling that of the far larger Christian population, as has previously been supposed, although communal administration was applied at district level.

A fair amount of scholarly attention has been paid in recent years to population statistics in the Ottoman province of Syria. Cohen has contributed more than his share to them, and in chapter 2 he refines his work further with data from the court records. How this population was administered by its own communal leaders is the subject of chapter 3.

Chapter 4 "Legacies, Debts, and Financial Obligations," together with chapter 7, "Economic Activity," shows clearly how much detail about economic history can be obtained from the court records. The Jews of Jerusalem were active in the full spectrum of the town's economic activities. And, like other groups in the empire—Muslim, Christian, and Jewish alike—they fell progressively further into debt as the century passed.

In matters of religion and social status, Jews fared as well as—perhaps better than—one might expect, living as a "protected" minority under a government legitimized by non-Jewish religious law. Certainly the Muslim government and courts protected them against the ubiquitous Christian "blood libel," yet the courts consistently viewed Jewish testimony as weaker than that of Muslims. Jews were tolerated but not treated as wholly equal subjects.

This book may be criticized on both large matters (for all the attention to economics, no attempt is made to factor inflation) and small (there are far too many typographical errors). Yet Cohen's picture of Jewish life is fascinating in its complexity, persuasive in its documentation, and intriguing in the questions it raises about the Judeo-Muslim cultural tradition of which this community was a small but significant part. It should stimulate similar, perhaps broader, studies of other communities of the period. At the very least, someone might begin by working on the other 80 to 90 percent of the population of Jerusalem itself—the non-Jewish part.

JON E. MANDAVILLE
Portland State University

YEHOSHUA BEN-ARIEH. *Jerusalem in the Nineteenth Century: The Old City.* New York: St. Martin's or the Yad Izhak Ben Zvi Institute, Jerusalem. 1984. Pp. xiii, 438. $29.95.

HAIM GERBER. *Ottoman Rule in Jersualem, 1890–1914.* (Islamkundliche Untersuchungen, number 101.) Berlin: Klaus Schwarz. 1985. Pp. 343.

Although covering some of the same time period and geographic region—Ottoman Jerusalem in the nineteenth century—these two books are vastly different in style, sources, and scholarly content.

Jerusalem in the Nineteenth Century is an attractive book, filled with period woodcuts and antique maps. Yehoshua Ben-Arieh has distilled the reports of travelers and others into a detailed description of Jerusalem, especially of the Old City. Travel literature has seldom been so completely exploited; on each page are several, sometimes ten or more, direct or indirect quotations from visitors to Jerusalem. The book considers every facet of life in the city from great matters, such as governmental administration, to small ones, such as where the lepers were kept.

For the reader simply curious about the life of the city, this book will be interesting and will provide an incredible amount of detailed information. For the historian, the book will be valuable but unsatisfying—valuable because it is a wonderful collection of useful facts and unsatisfying because it is only that. Ben-Arieh has written a descriptive, not an analytical, history. When reading this book one cannot help but wonder what was behind all the marvelous pictures and anecdotes. Why were things the way they were?

The lack of analysis will be a problem only to those who feel its need; purely descriptive history is surely an established genre. The book's main fault is a matter not of scholarly preference but of missing sources. Of the more than four hundred sources listed in the bibliography, not one is in Ottoman Turkish. The book provides statistical estimates from many European sources, but no mention is made of the Ottoman statistics of Jerusalem (unless they were quoted, and often distorted, by Europeans). This may not be quite the equivalent of writing a history of London exclusively from the works of Chinese travelers, but, even given the difficulties of Ottoman sources, it is nevertheless inexcusable. Ben-Arieh recognizes but does excuse this lack: "Very few Arabic works or Ottoman documents pertaining to the nineteenth century are now available. The only material published as of now deals with the end of the Ottoman period, that is, with the beginning of the twentieth century" (p. 11). Unfortunately, this is not true: Ottoman documents and published books are available. Sultan Abdül-

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hamid II even commissioned photographs of Jerusalem, which are readily available as are statistics of Jerusalem Sancak. The *1313 [1895] Istatistik-i Umumi*, for example, provides published information on Jerusalem's population, doctors and hospitals, schools, jails, and so forth. Many of the best published Ottoman statistical documents do indeed come from the early twentieth century. And, since Ben-Arieh does not hesitate to quote statistics from the mandate period, would it not have been reasonable to have used Ottoman documents as well?

Haim Gerber's *Ottoman Rule in Jerusalem* is sound historical scholarship. The title may mislead some readers into assuming that the book is a study of the city of Jerusalem. In fact, it is a significant analysis of the administration and economy of Jerusalem Sancak in the final years of Ottoman rule.

Gerber uses Ottoman documents and Arabic sources, as well as European reports, to create a balanced and accurate study of an area of history in which balanced, unprejudiced studies are badly needed. His description of the theoretical and actual governmental structure of Jerusalem Sancak is one of the best analyses to date of local administration in the later Ottoman empire. He examines the main areas in which Ottoman governmental policy affected the people of Palestine, including among the topics discussed land tenure, taxation, prices, production, trade, courts, and demographic change. From his analyses, Gerber draws conclusions on the relative success of Ottoman reform efforts. He does not hesitate to challenge some cherished beliefs about Ottoman rule, and in the process he explodes many myths. For example, by analyzing both land use patterns and linguistic evidence in Ottoman sources, Gerber advances a convincing argument against the "desertification" thesis, which assumes that during Ottoman rule "the land of milk and honey" gradually turned into a desert.

There are some problems with statistics, such as Gerber's use of an index based on plague deaths in Europe to calculate population figures from the number of plague deaths in Greater Syrian cities and his assertation that the population of Syria and Palestine more than doubled from 1800 to 1895. Nevertheless, Gerber generally shows a realistic skepticism in considering nineteenth-century statistical sources. His conclusions on increased urbanization and economic development are particularly well supported and reasonable.

The value of *Ottoman Rule in Jerusalem* goes beyond the analytical view it presents of Ottoman Palestine. Gerber shows that the Tanzimat reforms were a real success in Palestine. Nowhere is this more evident that in his discussion of legal reform, in which he demonstrates from court records and eyewitnesses that the new courts and a new law code had begun to provide real justice for Muslims and non-Muslims alike. This is significant because it shows that the new system was beneficial not only to the central government but also to Ottoman citizens. When such benefits are considered alongside improvements in commercial life and local government, it becomes obvious that Ottoman reforms created a better society for the citizens of Ottoman Palestine. Those who have long been able to see only the disadvantages of Ottoman rule should take note.

JUSTIN MCCARTHY
University of Louisville

JEHUDA REINHARZ. *Chaim Weizmann: The Making of a Zionist Leader*. New York: Oxford University Press. 1985. Pp. x, 566. $29.95.

Few serious biographies of outstanding Jewish figures have been written, and fewer still may be ranked as scholarly. No doubt this has something to do with the Jews' dispersion and also with a dearth of well-arranged, accessible documentation. The academic community will therefore be especially grateful to Jehuda Reinharz for this, the first of two volumes in the biography of Chaim Weizmann—a young radical Zionist in Russia, a key figure in the English Zionist movement (he settled in Manchester in 1904), an important chemist, and, following the issue of the Balfour Declaration in 1917, a leading tribune of the Jewish people and first president of the state of Israel, from 1948 until his death in 1952.

Reinharz's book is an academic tour de force, which makes use of some twenty-seven private and public archives, containing documents in five different languages (120 pages of footnotes at the end bear witness). Above all, this study rests on Weizmann's own correspondence, assembled laboriously at the Weizmann archives at Rehovot, Israel. Weizmann was a prolific, compulsive letter writer; during one nine-month period, from November 1902 to August 1903, Weizmann wrote nearly five hundred letters, in Russian, German, Hebrew, and French.

The biography of a man so closely associated with the Zionist movement must perforce be a history of that movement too. This biography has made excellent use of all the standard works. Reinharz's story follows Weizmann's peregrinations from the small *shtetl* of Motol to Pinsk, Berlin, Geneva, and, finally, Manchester. A detailed historical cameo of each stop is provided.

Like many great men, Weizmann had his personal weaknesses. He had a thin skin and took offense easily. He might relapse into depression without apparent reason or exaggerate his achievements to compensate for frequent bouts of self-doubt. Much of the detail in this book, including

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Weizmann's "roving eye," is published here for the first time.

The pre–World War I period, dealt with by this first volume, was a seminal one in which Weizmann's "personality, Zionist weltanschauung, and scientific direction were forged" (p. viii). Weizmann was a leading light in the Democratic faction, a short-lived, youthful opposition to the Zionist establishment. The faction was among the most vocal of the opponents to the so-called Uganda scheme put by Theodor Herzl to the Sixth Zionist Congress in 1903. Weizmann himself, Reinharz reveals, was initially ready to compromise (on Uganda) but soon got into line when he saw the extent of Russian opposition. Weizmann soon tired of futile opposition and devoted himself to cultural activity—a publishing house and the project for a Hebrew university (opened eventually in 1925 by Lord Balfour, with Weizmann presiding, in Jerusalem).

Once settled in Manchester, Weizmann devoted more time to his scientific career and the search for a marketable patent that would make him financially independent. By the outbreak of the war, he had also built up a circle of young intellectual disciples, who in the future would play a role in promoting Weizmann's ideas and leadership. (This so-called Manchester circle included two young businessmen, Simon Marks and Israel Sieff, who after the war would found Marks and Spencer.) To the English elite, Weizmann personified the authentic Eastern Jew. Whereas Herzl had gained entry to the highest circles by virtue of his reputation as a leading European journalist, Weizmann reached the same heights by virtue of his scientific achievements.

One of the most fascinating aspects of Reinharz's study is the exposure of some of the myths and errors propagated by Weizmann's memoirs, *Trial and Error* (1949). Perhaps the most striking example is Weizmann's later reconstruction of his own reaction to the notorious Kishinev pogroms in 1903. Weizmann was in Geneva at the time, having just returned from a visit to Russia. His memoirs contain an entirely fictitious account of a return visit to Russia to help Russian Jewry. This fantasy no doubt reflects an uneasy conscience over what was an inappropriate lack of reaction at the time.

With its erudition and detail, this is not always an easy book to read. But it must surely become the definitive biography of a man who was arguably the foremost representative of the Jewish people in the twentieth century.

MICHAEL J. COHEN
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AFRICA

DAVID W. PHILLIPSON. *African Archaeology.* (Cambridge World Archaeology.) New York: Cambridge University Press. 1985. Pp. ix, 234. Cloth $39.50, paper $14.95.

David W. Phillipson, an experienced archaeologist who has worked in Eastern, Central, and Southern Africa, has now written what the French call an *oeuvre de vulgarisation*—a work of scholarship intelligible to the general reader—which, for first-year university level, can serve students as a useful introduction to the subject. It is a distillation of the present state of our knowledge of African archaeology and will be required in every Africanist's library. It is also lavishly well illustrated.

Phillipson wisely cautions us not to place too great a reliance on linguistics and oral traditions and warns of the still random nature of our knowledge of African prehistory. The emergence of man in Africa, an intricate subject, is treated with great clarity, and so too is the confusing subject of the Acheulian Age, for which Phillipson compares tool-making complexes from Africa to Asia and to as far north as southern Britain. He traces the regional diversifications that elaborated as man, still a hunter and gatherer, developed more refined techniques. Burial of grave goods took place, indicating a belief that life continued after death. Refinement among the living is illustrated by items of personal adornment and the appearance of rock art, both painting and engraving, which Phillipson connects with the beliefs of the artists rather than with a desire to create objects of beauty. Some eighteen thousand years ago permanent settlement first began in the especially favorable conditions of the Nile Valley, followed by others in the lake regions of East Africa and in the Sahara, whose varying lake levels are not yet fully understood. Hunting and gathering doubtless continued to coexist with fishing, the domestication of animals and food plants, and the evolution of stone and pottery vessels.

Africa, outside Egypt, is the only continent that had no Copper or Bronze Age. The last two chapters describe the Iron Age before and after A.D. 1000 respectively. They are a tour de force of compression, for they include dynastic Egypt, Phoenician, Roman, and Vandal times in North Africa, as well as the arrival of the Arabs and Islam. In a period stretching from the *Periplus to the Erythraean Sea* to our own day, one must not complain too much of omissions. But Christian Egypt and North Africa were seminal not only in theology but also in the monastic movement in fourth-century sites still in current use. Nor can one complain that recent finds have already overtaken some sections: for example, the survey of forty-seven sites in Zanzibar and Pemba during 1984 disclosed the first hoard of silver coins found south of the equator and nine local rulers who were hitherto unknown. Rather, one hopes that the publishers will issue

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revised editions from time to time in order to maintain so useful a summary to keep abreast of the advance of knowledge.

G. S. P. FREEMAN-GRENVILLE
Sheriff Hutton, York

GABRIEL R. WARBURG. *Egypt and the Sudan: Studies in History and Politics*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1985. Pp. 253. $30.00.

Six previously published articles have been revised and extended to comprise this book, *Egypt and the Sudan: Studies in History and Politics*. The book is divided into two parts. The first, echoing Elizabeth Monroe's overview, is called "Britain's Moment in the Nile Valley" and includes four pieces. "From Conquest to Independence: 1882–1956" uses Ronald Robinson's theory of imperialism as a starting point to explain Britain's constant search for collaborators. Chapter 2, "The Sudan in Anglo-Egyptian Relations: 1899–1924," reviews the structure of British administration in the Sudan and details Anglo-Egyptian negotiations over the Sudan after World War I. Chapter 3, "The Sinai Peninsula in Anglo-Egyptian Relations, 1906–1947," is a neat exposition, to which two important documents concerning the status of the peninsula are appended. Chapter 4 gives a precise account of "The 'Three-legged Stool': Lampson, Faruq, and Nahhas, 1936–1944." Part 2 of the book, called "Regional and Political Considerations," contains two studies, "Egypt's Regional Policy in the Nineteenth and Twentieth Centuries" and "Islam and Politics in Egypt and the Sudan under the Military."

In dealing with the whole question of collaboration, Gabriel R. Warburg, like other scholars, may be unduly surprised by the ease with which the British managed, without numerically strong armed forces, to protect their position in the Nile Valley. In the widest sense, collaboration is the basis for all rule and is just a special form of consent. Besides, in the Sudan especially, history, ethnicity, geography, and technology all favored the imperialists. The ultimate problem of collaboration was how to maintain the collaborators' position without allowing them to threaten the imperialists' position. As Lord Killearn put it:"'To sit back and remain splendidly aloof from Egyptian internal politics sounds extremely attractive in theory. . . . But with Egypt situated where she is, how long will that be possible?'" (p. 34). Thus, it was repeatedly necessary to reenter the fray, shift alliances, bargain and threaten, and prevent the emergence of either an opposition strong enough to resist British force or a collaborating element strong enough to do without British support.

The Sudan's position in Anglo-Egyptian relations was a point of contention throughout Britain's period of control of the Nile Valley. Warburg's discussion in chapter 2 is an excellent introduction to the subject, although some of his conclusions may be questioned, as, for instance, his rather indulgent view of Austen Chamberlain's role in the 1924 crisis (p. 77). "The unity of the Nile Valley," for long a battle cry of Egyptian nationalists that was eventually echoed by anti-imperialists in the Sudan, is a theme that, under detailed examination, looks less convincing than its basis in cultural and geographic similarities at first suggests. This is borne out in Warburg's exposition of the role of Islam in the two countries. Yet, in dealing with Egypt's "regional policy" since Muhammad Ali, he seemingly accepts that the "ultimate integration" of Egypt and the Sudan is favored by the Sudanese (p. 186), a view that fails to take account of the hostility of, among others, Southern Sudanese to the Integration Charter and other grandiose schemes. That opposition is clearly indicated, however, in the book's final sentences: "Numayri's announcement in September 1983 that the *hudud* as prescribed in the Shari'a would be applied to all Sudanese Muslims seems to be another indication of the paramount position of Islam in Sudanese society and politics. Yet with the resumption of civil war in the South, and the economy on the verge of bankruptcy, this may well be an act of desperation" (p. 237). These are strong words and true, but the Sudan, like Egypt, also has a secular tradition. In both countries it is recognized that political debate and the vexed question of nationality must be freed from the opportunistic appeal to religious feeling.

This book solidifies Warburg's reputation as a meticulous and thoughtful scholar who has made excellent use of his unfortunately limited sources to produce detailed and valuable special studies. It is convenient to have six of these now available in a well-produced volume.

M. W. DALY
Arkansas State University

ROBERT O. COLLINS and FRANCIS M. DENG, editors. *The British in the Sudan, 1898–1956: The Sweetness and the Sorrow*. (St. Antony's/Macmillan Series; Hoover Press Publication, number 302.) Stanford: Hoover Institution. 1984. Pp. xxii, 258. $24.95.

This volume of essays is edited by Robert Collins, a widely recognized American expert on the Sudan, and Francis M. Deng, a Sudanese scholar-diplomat. Other essays are contributed by Collins's former students at the University of California, Santa Barbara: Michael Coray, John Frost, Gail Schoettler, Thomas Fuller, and Jack Bermingham. An appen-

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dix, by A. H. M. Kirk-Greene of St. Antony's College, Oxford, systematically analyzes the educational backgrounds of members of the Sudan political service.

Apart from eight in-depth interviews with retired Sudanese officials, which form the basis of Deng's essay on Sudanese perceptions of Anglo-Sudanese relations, the essays draw primarily on impressive caches of memoirs and contemporary diaries and letters of British officials at the School of Oriental Studies, Durham University, and Rhodes House, Oxford. The emphasis is on the British: their attitudes, work routines, social and family lives. In general, the thesis is that the British may have had faults (especially their failure to establish close relations with the educated Sudanese who some officials realized would soon be running the country) but that in the main they were conscientious and dedicated to duty and service. The postcolonial world has condemned them and their work too easily. Most of the chapters deal with the political service, but the technical departments (railways, medical, education, and so forth) and missionaries get a chapter apiece. An introduction by Collins deftly sets out the geographical, demographic, and historical context of the complex country and its evolution under British rule. In view of the recent political and famine crises, the conclusion that the British succeeded in leaving behind a working democracy that is economically viable seems somewhat optimistic.

Although the preface tries to establish the broad theme of cultural contact, the publisher's blurb, for once, is more accurate, describing the book as essentially "raj nostalgia." The tone is light, even gossipy; this is a good read, although the student of colonialism will find little that is either suprising or new. This is not a deep examination of the British impact on the Sudan, but it was not meant to be.

JOHN W. CELL
Duke University

ASIA AND THE EAST

HOWARD J. WECHSLER. *Offerings of Jade and Silk: Ritual and Symbol in the Legitimation of the T'ang Dynasty*. New Haven: Yale University Press. 1985. Pp. xiv, 313. $27.00.

Howard J. Wechsler's absorption in late Sui and early T'ang (roughly, the seventh century), having already yielded one well-received book and several useful articles, has now generated an important analysis of how the early T'ang rulers manipulated ceremonial matters for purposes of legitimation. It is a gracefully written, well-made book in a field too long underdeveloped, especially since governing China was traditionally conceived of, largely, as a ritual business.

In preparing for this work, Wechsler commendably took advantage of an opportunity to spend one semester studying a second discipline, anthropology. This enabled him to make effective use of relevant theoretical constructs, not yet absorbed by most historians of China, that have been developed by such cultural anthropologists and other social scientists as David Easton, Amitai Etzioni, Jack Goody, Abner Cohen, Edmund Leach, Peter Berger, and Mary Douglas.

Wechsler has not, however, organized his book along the analytical lines of Easton's "specific support/diffuse support" or Etzioni's "coercive compliance/utilitarian compliance/normative compliance" or the concepts "credenda" (things to be believed) and "miranda" (things to be admired). Rather, his chapters principally analyze rituals and court decision making pertaining to them, one by one: portents, accession, suburban sacrifices to heaven, ancestral cult ceremonies, activities relating to imperial tombs, imperial tours of inspection, rare Feng and Shan sacrifices, activities relating to the Ming-T'ang (Hall of Light), and promulgation of the calendar. Besides describing in detail what happened, Wechsler carefully traces the history of the issues in a fine display of Sinological erudition. Each chapter is a neat essay and will be a valuable reference.

Wechsler's main theme is that early T'ang, because of its ritual-related decisions, should be considered a watershed in the evolution of Chinese conceptions of the state and nation. In addition to choosing the doctrine of *t'ien-hsia wei kung* (the empire is open to all) over that of *t'ien-hsia wei chia* (the empire belongs to one family), the early T'ang rulers downgraded lineal ancestors to the greater glory of "political ancestors," including pre-T'ang heroes. As a corollary, they began to incorporate into the imperial family honored contemporaries who were not kin, thus transforming the kinship-defined imperial family into a "political family." The rulers also preferred to worship a single, universal heaven rather than multiple deities and emphasized the ruler's exclusive right to identify with heaven. This combination of choices, in Wechsler's view, was a significant step toward intensified autocracy in later times. It is an important suggestion, and Wechsler's presentation of it, though appropriately tentative, is persuasive.

CHARLES O. HUCKER
EMERITUS
University of Michigan

JONATHAN D. SPENCE. *The Memory Palace of Matteo Ricci*. (Elisabeth Sifton.) New York: Viking. 1984. Pp. xii, 350. $19.95.

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It is a mark of our growing interest in and understanding of the historical interpenetration of Chinese and European cultures that this book should appear. Thirty years ago, it is doubtful that a historian of Jonathan D. Spence's stature would have undertaken such a study. Although most of the primary sources were then available, the secondary literature, including works from Taiwan since the 1950s and from mainland China since 1979, had only just begun to generate the interest in a cultural intermediary like the Jesuit missionary Matteo Ricci (1552–1610). The author has carefully studied this growing body of material and has produced a finely written and absorbing portrait of Ricci that appeals to both academic and nonacademic audiences.

As has become typical of Spence, this work is distinguished by a far greater aesthetic dimension than is usual in works by contemporary scholars. The work is structured around Ricci's interest in European mnemonic traditions, which were then entering their final historical phase. These mnemonic techniques, which used vivid spatial ordering in the mind ("the memory palace") to expand the ability to commit information to memory, helped Ricci dazzle the Chinese with his powers of memorization. The author builds the book around four memory images drawn from *Jifa*, Ricci's Chinese treatise on mnemonic arts, which plots the simple beginnings of a memory system. Ricci placed each of these images in a respective corner of an imaginative entry-room to a memory palace. To these four images the author adds four religious pictures drawn from the Bible. These holy pictures, with captions and commentary by Ricci, were preserved through their publication by the Chinese inkstone connoisseur Cheng Dayue in 1606. Rejecting a straightforward chronological framework, the author constructs Ricci's biography by interweaving biographical material with these eight elements. This structure allows for the creation of a vivid picture of the post–Counter Reformation world in which Ricci lived.

The excellence of this book lies in its remarkable execution that blends art and scholarship in a way that few scholars could emulate. His study deepens our knowledge, though it does not produce any fundamental reinterpretation of either Ricci and the China Jesuits or our understanding of Chinese history. The author's handling of the sources is impressive. One criticism I would make is that he sometimes slights the group context in which Ricci worked. It is one-sided to dismiss Nicolas Trigault's editorial changes of Ricci's *Historia* manuscript as "distortions, deletions and additions" (p. 271) or as mere impediments to understanding the work. Although such an approach allows us to sort out Ricci's individual contributions, it does not give a full picture of the historical conditions in which the work was produced. This was a group effort that Trigault, like Ricci before him, was ordered by superiors to undertake for the sake of the Christian mission in China. Without this stimulus, the *Historia* probably would never have been written and, in the Ricci-Trigault edition, would never have become the most widely read European book on China of its time. But there are not many criticisms that I can make of this study. Spence is a twentieth-century master of his art, and he has given us a masterpiece.

DAVID E. MUNGELLO
Coe College

CHARLES O. HUCKER. *A Dictionary of Official Titles in Imperial China*. Stanford: Stanford University Press. 1985. Pp. xiii, 674. $49.50.

A novice student of Chinese history encounters four difficulties: time, place, biography, and official titles. How to convert a date from the Chinese to the Western calendar, how to identify the modern name for an ancient place, and how to locate a person in historical sources have been largely overcome by reference tools. But how to translate Chinese official titles of all dynasties into English has not been easy until the appearance of this dictionary. This reviewer has been dreaming of such a reference book for half a century, and now the dream has been realized by Charles O. Hucker, who has made a signal contribution to Sinology.

This book, which covers the eleventh century B.C. to A.D. 1911, is preceded by an introduction to governmental organization, era by era. After presenting an organizational chart for each dynasty, Hucker highlights the central government, territorial administration, military organization, and personnel recruitment. The main body of the dictionary contains 8,291 individual entries for titles, institutional names, and related terminology arranged alphabetically by Wade-Giles romanization. Each title, followed by Chinese characters, is given a "suggested" English translation, indicating the historical periods of its usage and comparative information for clarification. The volume concludes with an alphabetic index for English renderings, another for Chinese characters, and a conversion table from Pinyin to Wade-Giles making the book very easy to use. The narrative is comprehensive, informative, and succinct.

This dictionary is not, however, all-inclusive and flawless. For example, there is no entry for the *chiu-p'in chung-cheng* (the nine-rank recruitment system by the so-called impartial referees). *Ch'u-shen* (to have come out) should be personal background or official qualifications. That "officals serving in the capital ran from 700 bushels down to 10 bushels a year" (p. 36) is improbable, because no official could

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live on ten bushels of grain annually. Equally inadequate is the translation of "1829: *erh-ch'ien shih ts'ao*," the section for 2,000-bushel officials, which should be 2,000 piculs as Homer Dubs correctly understood. The character *shih*, often pronounced *t'an*, means a weight of 120 catties; that is a picul. Equally dubious is the statement that "in Northern Sung times every circuit was expected to maintain a fleet" (p. 48). The term *chieh-tu shih* is translated by Hucker as "military commissioner," by William Hung as "governor-general," and by Denis Twitchett as "military governor." *Tsai-hsiang* is rendered by Hucker as "grand councilor" and by Twitchett as "chief minister." Therefore, in a revised edition, it would be helpful to add a long list of reference works similar to those appended to the biographical dictionaries of the Ming, Ch'ing, and the Republic periods. It should include, *inter alia*, Edward Kracke's *Translation of the Sung Civil Service Titles* (1957), W. F. Myers's *The Chinese Government: A Manual of Chinese Titles* (1897), K'ang Yu-wei's *K'ang Nan-hai kuan-chih i* (K'ang's Comments on the Official System [1904]), and Ch'üT'o-yüan's (Ch'ü Hsüan-ying) work in the appendixes of Huang Pen-chi's *Li-tai chih-kuan piao* (Tables of Official Titles of All Dynasties [1965]). These reference works could inspire a serious-minded student to compare, do further research, take a choice, or make a better translation. Thus, the usefulness of this monumental work will be further increased.

S. Y. TENG
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FERNANDO GALBIATI. *P'eng P'ai and the Hai-Lu-Feng Soviet*. Stanford: Stanford University Press. 1985. Pp. vi, 484. $45.00.

Mao Tse-tung is generally regarded as the great innovator and leader of peasant revolution in China, but the first significant Communist organization of Chinese peasants was undertaken by P'eng P'ai several years before Mao turned his attention to the countryside. P'eng, scion of a Kwangtung gentry family, became a radical peasant leader even before he joined the Communist party in 1923. In 1922 he organized peasant unions in eastern Kwangtung, particularly in the two counties of Haifeng and Lufeng (commonly abbreviated as Hailufeng). The union successfully undertook a number of welfare and rent reduction actions and expanded its influence through much of Kwangtung province, leading to bloody confrontation with the landlords and militarists.

During the period of cooperation between the Communists and the Kuomintang from 1924 to 1927, P'eng led the way in organizing the peasantry and training peasant leaders under the Kuomintang flag. After the collapse of the Communist-Kuomintang coalition, P'eng returned to Kwangtung and created a revolutionary base, the Hailufeng soviet. The soviet government confiscated and redistributed the land, burned land deeds, launched a red terror against "class enemies," and tried to create an egalitarian and structured social order. It was the first attempt at mass-based agrarian revolution and the establishment of a "democratic dictatorship" in China. The soviet lasted only for about three months, from November 1927 to February 1928, before it was destroyed by Kuomintang armies. In the following year, the Kuomintang captured P'eng and executed him.

Fernando Galbiati's book is the first comprehensive and detailed study of this subject in English. The title is not inappropriate but is nonetheless a bit misleading. The book is not a full-fledged biography of P'eng P'ai by any means, and only the long penultimate chapter deals with the Hailufeng soviet. Above all, the book is a study of how P'eng and his associates mobilized peasants in eastern Kwangtung. A major theme in the book is the relationship between communist ideology and goals, on the one hand, and the goals and assumptions of the peasants, on the other. Mobilization was successful only when there was some measure of congruence between these two, but it was never complete. The Communist leaders thought in ideological and political terms but had to accommodate peasant values and aspirations. The peasants chanted Communist slogans but were really only concerned with traditional values and aspirations that were local and, in Communist terms, "ultra-democratic" or, in some cases, downright reactionary.

The book is based on an extraordinarily wide range of Chinese and other sources and provides an enormous amount of fascinating detail about conditions in Kwangtung, peasant union policies and problems, actions of the Hailufeng soviet government, and myriad related topics. Unfortunately, Galbiati does not manage the detail as well as he might; he seems to have little notion of summary, emphasis, variety, and generalization and tends to inundate the reader with an endless succession of facts. But the other side of the coin is that the facts make this a valuable book that will not be soon superseded. It will be of great interest to everyone interested in peasant mobilization and peasant revolution.

JAMES E. SHERIDAN
Northwestern University

B. B. MISRA. *District Administration and Rural Development in India: Policy Objectives and Administrative Change in Historical Perspective*. New York: Oxford University Press. 1983. Pp. xiii, 431. $29.95.

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This book is designed to complement B. B. Misra's earlier books on cognate subjects: *The Central Administration of the East India Company, 1773–1834* (1959) and the *Administrative History of India, 1834–1947* (1970). It covers the evolution of district administration as conceived by the top colonial authorities from the beginnings of British rule in Bengal until 1947. Misra, in fact, also purports to analyze the developments in the period from independence to the end of the 1970s, but he manages to give only a compressed and highly selective history of administration in more recent times.

The book represents an official history of the kind that used to be penned by high officials of the Indian government or of the East India Company. There are few references to secondary works published after independence. The truth, in Misra's view, is embodied in official or semiofficial dispatches of governmental officials or other policy makers, and humbler voices can be safely ignored.

Such an account has its virtues, not the least of them being clarity. The chronology of changes in the functions of revenue collector, judge, and magistrate and the motives for changes in these functions (including their union in a single person or dispersion between different functionaries at the same or at different levels of government) are lucidly presented.

But Misra's method effectively precludes an analysis of the real impact of the changes described on Indian society, whether viewed from above or from below. He strings together the story of the administrative changes by means of casual explanations offered by the administrators. So the history of district administration and rural development becomes only an administrators' history of administration and rural development. Indeed, as far as rural development is concerned, Misra does not even offer a proper account of the broader events such as famines and the clash of opinion among the administrators themselves. Misra makes little mention of the sharp conflict between George Campbell and other British policy makers as regards famine relief; likewise, William Digby, James Geddes, and other dissenting policy makers are not mentioned anywhere in the book.

With such limitations of interpreting the perspective of the policy makers themselves, it is futile to expect Misra seriously to confront or digest the findings of numerous Indian and foreign historians regarding the seething contradictions within Indian rural society or the diverse impact of the British imperial rule and the international economy in retarding the Indian economy. Misra indulges in snap judgments without providing explanations. For example, according to Misra, Indianization of the services in the twentieth century led to a decline in "efficiency," but he does not define what he means by "efficiency." Nor does he pause to ask whether Indianization might not have been forced by the progressively higher opportunity costs of technical or administrative cadres recruited from Britain.

The book has the appearance of being rather hurriedly written. There are numerous misprints, tables that illuminate little and are sometimes nonsensical (for example, in the table on page 153 the average size of a district in Bengal is given as 116 square miles and in Uttar Pradesh as 107 square miles), and a bibliography that leaves a lot to be desired. This book might still be useful to those unfortunate examinees who have to memorize the chronology of file-pushers to pass their examinations and, if cautiously used, as a ready reference for a narrative of purely administrative changes (but not rural development) at the district level in British India.

AMIYA BAGCHI
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RUDRANGSHU MUKHERJEE. *Awadh in Revolt, 1857–1858: A Study of Popular Resistance*. New York: Oxford University Press. 1984. Pp. xvi, 219. $24.95.

At once a far-reaching challenge to the complacent supremacy of the British and the sole armed uprising against their rule, the Indian Revolt of 1857 has provoked intense scrutiny and, not surprisingly, a great deal of controversy. Among the legion of books on the subject, however, few have taken as their focus the revolt in Awadh (anglicized as Oudh after the state's 1856 annexation to British India). Even Eric Stokes, in his meticulously detailed studies of the rising, examined only the western districts, toward Delhi, of what is now Uttar Pradesh. Yet Oudh was, after Delhi, unquestionably both the focal point of the rebellion and the scene of the most tenacious fighting. Only in December 1858, a full year and a half after the initial outbreak, were the last embers of revolt extinguished. So Rudrangshu Mukherjee's careful account of the uprising in Oudh is a welcome addition to the literature on the Mutiny.

In his introductory chapters Mukherjee lays out the agrarian background to the revolt. Taking his cue from the "moral economy" notions of E. P. Thompson and James Scott, he sees landlord and peasant in Oudh bound together by structures of "interdependence and complementarity" that provided a basis for united action in times of crisis (p. 51). He then goes on to argue that annexation brought with it a "veritable rupture" in the traditional order of things and so laid the foundation for the subsequent revolt (p. 59). He even attempts,

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though not wholly successfully in view of the limited data available, to establish a correlation between assessment levels, dispossession, and participation in the rebellion for the landlord-dominated eastern and northern districts. His similar attempt to link the military mutinies with the discontent in the Oudh countryside, though suggestive, is hindered by the lack of information regarding recruitment patterns.

The heart of the volume lies in Mukherjee's detailed narrative account of the Oudh uprising, which he calls a "revolt of the people." Throughout, he concentrates not on the British but on the various rebel groups, whose activities, he maintains, were marked by a high level of unity, planning, and coordination. He insists, in particular, that an organized government came into existence in Lucknow under Begam Hazrat Mahal, that it secured at least the partial allegiance of the sepoys and landed taluqdars, and that, if sometimes ineffectual, it nevertheless maintained "a semblance of order" in the rebel-held areas until the bitter end (p. 143).

Mukherjee does not attempt the hopeless task of trying to make the revolt in Oudh part of the subsequent larger "freedom struggle." In common with most recent scholars, he sees the uprising as an effort, fueled by fear for religion and led by traditional elites, to restore the old "moral economy" (p. 169). Nevertheless, his emphasis on the popular character of the revolt, together with his insistence that a "widening of linkages" created a "unified opposition" (pp. 162–63), demarcates Mukherjee's analysis sharply from that of such scholars as Stokes. He refuses, above all, to accept caste and factional alignments, or more generally any "narrow" conceptions of self-interest, as decisive in informing behavior during the Mutiny. For him what matters is not whether loss of land or loss of lordship propelled an individual taluqdar into rebellion. It is sufficient that British policy was "from the outset" consistently anti-taluqdar and so had a "shattering impact" on the world of rural Oudh (p. 161).

Much in Mukherjee's analysis is compelling. Certainly, the widespread character of the uprising in Oudh is unquestioned. Yet it is by no means certain that a single year of British rule had wholly shattered the "moral economy" of the countryside. The destruction of the taluqdars' forts, the most visible mark of their lordship, for instance, had hardly begun when interrupted by the rising. In the adjacent North-Western Provinces, as the recent research of C. A. Bayly has made clear, no "rupture" in the traditional order took place until the depression of the 1830s, some three decades after annexation.

One cannot, as Mukherjee does, simply disparage, as an "otiose exercise in disaggregation" (p. 161), a search for individual motive among the rebel leaders. In the North-Western Provinces in particular, where half a century of economic and administrative changes intersected in rural society so that cross-cutting patterns of loyalty and rebellion emerged, only a careful probing of motive can make clear the nature of the upheaval. Then, too, with a state structure of its own until a year before the Mutiny, Oudh possessed a focus for unified action inconceivable in a disparate set of districts strung along the Ganges-Jumna Doab. Committed to a "populist" view of the Mutiny, Mukherjee seems perhaps not to appreciate the exceptional character of the revolt in Oudh. With this work Oudh is at last fitted into that thoughtful reconsideration that Stokes began some fifteen years ago. But the traumatic events of 1857 will continue still to challenge historians of modern India.

THOMAS R. METCALF
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K. N. CHAUDHURI. *Trade and Civilisation in the Indian Ocean: An Economic History from the Rise of Islam to 1750*. New York: Cambridge University Press. 1985. Pp. xiv, 269. Cloth $42.50, paper $16.95.

KENNETH R. HALL. *Maritime Trade and State Development in Early Southeast Asia*. Honolulu: University of Hawaii Press. 1985. Pp. xv, 368. Cloth $25.00, paper $14.95.

One has to admire the historian who attempts a broad treatment of Southeast Asia as a region, especially in its pre-European phase. The study of Southeast Asia's early history presents a particularly difficult challenge because of the paucity of archaeological, epigraphical, and literary evidence available, which has inclined historians toward much theorizing and speculation. Yet real progress has been made during the past forty years or so toward working out an acceptable construct of the region's history.

The pioneering work of the French scholar George Coedes and the more recent studies of such writers as Paul Wheatley and Oliver Wolters have carefully reexamined and reinterpreted the scattered sources of Southeast Asia's early history. Kenneth R. Hall's book reflects some of the new orientations and perspectives that have emerged as a result. He succeeds in bringing together new theoretical constructs and recent archaeological finds and interpretations so as to demonstrate general regional patterns.

He identifies two different models of exchange and statecraft in early Southeast Asia: the decentralized riverine system, where the focus of trade was a coastal base located at a river mouth, typified by the

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south Sumatran state of Srivijaya, and the more integrated wet-rice plain system characteristic of the states of mainland Southeast Asia and Java. In both cases, however, manpower rather than territory was the basis of political control. Also, both types of state emerged mainly as a response to developing international trade. The "rise and fall" of early Southeast Asian states, the changes that occurred in the direction of the main maritime trade routes, and the strength of international demand for typical Southeast Asian products, such as spices, were all closely interrelated phenomena.

The southern shift of the main international maritime route between India and China, from the Isthmus of Kra and the south Vietnamese coast to the Strait of Malacca and the Java Sea around the fifth century A.D., opened up a new strategic commercial zone and led to the emergence of major commercial centers in Sumatra and Java. The successful state was one that made the best use of its location on the trade routes to combine the provision of good port facilities for foreign traders with the efficient organization and marketing of local produce.

Another common characteristic of virtually all early Southeast Asian states was their assimilation of Indian cosmological symbolism and religious theory to form an ideological basis for kingship. Indian religious ideas and ritual were consciously adopted to reinforce the ruler's political and economic aims. This was part of a process of acculturation, an example of the typical Southeast Asian interplay of external influence and indigenous tradition.

The main theme of Hall's book is therefore that the interaction between early Southeast Asian peoples and foreign cultures resulted from the commercially strategic position of the region in relation to the major premodern maritime routes between India and China. The author's view is that the Southeast Asian response to international trade in this period was a reflection of preexisting patterns of exchange and amounted to the adaptation of the already well developed internal socioeconomic and political networks to new external influences.

Hall's study portrays aspects of Southeast Asian history on a large canvas, but K. N. Chaudhuri's book covers an even broader geographical continuum. Inspired by Fernand Braudel's concept of the Mediterranean as a physical and human unit of civilization, Chaudhuri has undertaken a discursive study of the cultural and economic roles of long-distance trade—incorporating shorter voyages and distances—between the eastern Mediterranean and the China Sea from the rise of Islam in the mid-seventh century to the beginning of European imperialism in the 1750s.

He perceives an underlying cohesion in the Indian Ocean, bound by seaborne trade and supplemented by the Central Asian caravan routes that together created a strong sense of unity. Unity was never superimposed. Southeast Asian states such as Srivijaya and Majapahit may have exploited the commercial advantages of their location in relation to international trade routes, but before the arrival of the Portuguese at the end of the fifteenth century no organized attempt had been made by any single power to dominate the long-distance trade of Asia. Neither India nor China showed any real sustained interest in maritime supremacy, and the long-distance trade of Asia remained a matter of individual or group mercantile capitalistic enterprise.

Linkages between long-distance trade, commercial capitalism, and production for the export market remained strong during the period. But the development of trade was influenced more by demand factors than by those of supply and production. The medieval trade of Asia was founded on the economic and social demand for the four great products of Eastern civilization: silk, porcelain, sandalwood, and black pepper. The technology of production remained constant, and change came as a result of shifts in demand or in the processes of exchange.

Both these books cover familiar ground, but they succeed admirably in opening up new perspectives and proposing fresh interpretations, thus helping to enrich the study of important aspects of Asian history.

BRIAN HARRISON
St. Paul's Bay, Malta

BENJAMIN A. BATSON. *The End of the Absolute Monarchy in Siam.* (Southeast Asia Publications Series, number 10.) New York: Oxford University Press, for the Asian Studies Association of Australia, Canberra. 1984. Pp. xviii, 349. $12.95.

It is fitting to review Benjamin A. Batson's book fifty-three years and sixteen coups or coup attempts (as of this September) after King Prajadhipok conceded the absolute monarchy in the first coup on June 24, 1932. At that time, although economic conditions throughout the world made instability common, the Thai coup took observers by surprise. But perhaps the greatest surprise was the ease with which Prajadhipok accepted the action and stepped down to become Thailand's first constitutional monarch. He viewed the change as a move in the right direction, something he had been contemplating for years but had postponed fearing that Thailand was not yet ready. It should be noted that he was quickly disillusioned with the performance of the new government and his lack of influence. He abdicated, retiring to private life three years later, in March 1935.

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For most contemporary observers of Thai history and politics, the end of the absolute monarchy in Thailand was little more than a logical development growing out of the changes in the internal structure of the country that had taken place during the reigns of Kings Mongkut (1851–68) and Chulalongkorn (1868–1910) and thereafter. As extensive as the royal family was, it was too small to provide adequate administration for the country. A new bureaucracy made up of commoner military and civilian personnel emerged and, naturally, in time took over. Indeed, it is this bureaucracy that governs Thailand today. Most contemporary writers give the 1932 changeover slight attention, concentrating instead on the factional struggles within Thailand's current leadership. (It is noteworthy that the leaders of last September's coup have been encouraged to leave the country quietly.) Fred W. Riggs covers the period in one line, David Wyatt in little more (p. 1). Batson, by contrast, expends 349 pages on the background and ultimate demise of the absolute monarchy, and those pages are exceedingly well used.

The real attraction of Batson's study is that it fleshes out the account and, through judicious use of detail in text and footnotes, makes the individuals of his narration human and understandable. Two examples should suffice. First, Pridi Phanomyong, then a student troublemaker in France unhappy with student allowances and active in student grievances, was ordered home by Prince Charoon, the Thai minister in Paris. On petition from Pridi's father, King Prajadhipok intervened on behalf of the youth and permitted Pridi to finish his studies in France (p. 79). It is this youth who later led the successful coup against the king. Second, when the French in Vietnam demanded the deportation of eleven Vietnamese antigovernment refugees to Indochina, the Thais arranged through a "bureaucratic 'misunderstanding'" to ship them off to China instead—thus vastly irritating the French (p. 176).

As a Burma/China specialist and a Southeast Asian generalist, I would probably have missed Batson's book had I not been asked to review it. I am grateful for my good fortune and recommend this reading to those of us Asianists who wish to be brought back in touch with Thai realities of the 1920s and 1930s.

JOHN SEABURY THOMSON
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ROSS FITZGERALD. *From 1915 to the Early 1980s: A History of Queensland*. St. Lucia: University of Queensland Press. 1984. Pp. xvi, 653.

P. F. DONOVAN. *At the Other End of Australia: The Commonwealth and the Northern Territory, 1911–1978*. St. Lucia: University of Queensland Press. 1984. Pp. xv, 277. $35.00.

Both for those who do and for those who do not know Australia well, these two books provide an important historical and interpretive service. P. F. Donovan and Ross Fitzgerald have written previous historical works on Australia, Donovan on the Northern Territory and Fitzgerald on Queensland. Here again they bring a considerable richness of sources and detail to bear on what are integral parts of the great variety that constitutes Australia—parts that are often dismissed as peripheral or unrepresentative.

Some find it tempting to ignore the Northern Territory as a curiosity: a huge and mostly parched area in the middle of the Australian mainland, conspicuously underpopulated, not a state, and only recently enjoying self-government. Unlike the states, the Northern Territory is a region whose governance has until recently been in the hands of frequently changed and usually low-status federal ministers and appointed administrators. Yet the Northern Territory and the state of Queensland can in important ways be compared. Like the Northern Territory, Queensland covers a great expanse of land. Unlike the southern Australian states, it is not focused on a single, overbearing capital city. It has emphasized pastoral and resource industries and, despite setbacks, has cultivated a mystique as well as economic policies subsumed under "development."

Despite Australia's inordinate urbanization and the multifaceted cosmopolitanism of Sydney, Melbourne, and elsewhere, the nation's overseas income derives predominantly from what is grown on or extracted from the land. Much of this wealth comes from Queensland and the Northern Territory, which, after decades of struggle and frustration, are in their own ways Cinderellas, attracting big money, big Australian and overseas enterprises, and internal migrants and tourists in large numbers.

Both regions had and continue to have a frontier quality that is reflected in popular and political style as well as in economic policies. The older, more established southern Australian states, and especially the federal government, are seen as remote in their appreciation as well as in distance. The Northern Territory's and Queensland's composition are also different from that of the south. Both have disproportionately high Aboriginal populations. The Northern Territory's multiracialism, which extends beyond its Aboriginal component, and Queensland's very low proportion of Continental Europeans have contributed to a certain ethnocentrism—proud and special or parochial, depending on one's vantage point.

These and other circumstances have bred a political style that is bold, brash, and, at times, raucous.

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Queensland, whose political career reaches back generations, has experienced inordinately long stretches of governance under both Labor and non-Labor parties. But Fitzgerald finds little meaningful difference in the way in which stability and growth have taken precedence over people and ideas. Echoing other critics, he concludes that a cleaving to material progress has stifled pluralism and has led to widespread complaints about censorship, repression of dissent, environmental indifference, corruption, and paternalism toward Aborigines. Yet the present premier, Johannes Bjelke-Petersen, would argue that, despite the gerrymander that helps prop up his National party government, in the last resort the Queensland electorate believes in and votes for him.

Donovan's account of the Northern Territory stops in 1978, when the territory achieved substantial self-government. The territory inevitably continues to be far more reliant on the cooperation and largesse of Canberra than is Queensland. A railway link across the full length of the territory remains unbuilt and will not be built until federal resources are committed. Over a quarter of the territory's land has, by federal enactment, been transferred to Aboriginal freehold, and there is legislative provision for even more transfers. The government of the Northern Territory has sought unsuccessfully to have this legislation tempered, especially to foster exploitation of natural resources. Statehood itself, though once regarded as an early and virtual certainty, has become a political issue nationally, entailing conflicts about human and economic programmatic priorities as well as about funding.

Like Queenslanders, most non-Aboriginal Territorians regard themselves as quite special and distinctively fortunate Australians. They are more assertive about their qualities and prospects, and their political style reflects this. The ethos of contemporary Queensland and of the Northern Territory, rooted far back in the present century, has been ably traced in these two useful books.

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ROGER B. JOYCE. *Samuel Walker Griffith*. Lawrence, Mass.: University of Queensland Press. 1984. Pp. xi, 456.

Samuel Walker Griffith was a man who rose from a lower-middle-class Welsh background to become premier of Queensland, a leading exponent of Australian federation, and the first chief justice of the High Court of Australia. The account of such a life ought to be both interesting and instructive, but this biography fulfills only the latter expectation.

Perhaps one of the main problems confronting the late Roger B. Joyce was the sheer bulk of the material he had gathered. In the preface we are told that the first draft of the manuscript came to a mammoth 780,000 words. Mercifully, this was finally reduced to a more than generous 200,000 words, and yet Joyce also admits to finding Griffith a difficult subject for biography: "I am not certain if I fully understand the 'strange' man with whom I have been living so long." The sentence preceeding this one is even more revealing of the challenge Griffith presents to those who would unlock the secrets of his psyche: "I believe there is enough here to show the public man and the times in which he worked, even if his inner self remains partly hidden" (p. x). As far as this reviewer is concerned, Griffith's inner self remains completely hidden, even after 200,000 words of exposition. Yet Joyce does recognize that Griffith lived "a passionate inner life often far removed from the cold outer existence imposed by his master the law" (p. xi). One can only regret that so little of this side of the chief justice remains in the biography.

What does survive is a detailed study of law and politics in colonial Queensland and the early years of the new federation. Joyce is at his strongest when he uses Griffith as a focal point from which to analyze Queensland politics and the movement toward federation. Here his touch is sure and his judgments persuasive and compelling. But, when he moves to a closely detailed account of the many law cases in which Samuel Griffith was involved, the reader's attention begins to wander and the book becomes heavy going. Joyce makes little attempt to relate these law cases to Griffith's social conscience or to the social milieu in which he and his associates moved. Instead, we have a plethora of detail in which one suspects that the material has taken over from the biographer, who appears to be unable to tell "the wood from the trees." Joyce was undoubtedly one of the best modern Australian historians when it came to commenting on and analyzing case law and its implications for the understanding of history, but here that very strength has unbalanced the biography.

Griffith's career is covered in considerable detail, as he moved from a scholarship boy at Sydney University through legal practice as a barrister in Queensland and into politics where he served in a variety of positions ranging from attorney general, to leader of the opposition, and ultimately premier of Queensland. In 1893 he resigned as premier and received a commission as chief justice of the Queensland Supreme Court. After being principal draftsman of the new federal constitution, Griffith became the first chief justice of the new High Court of Australia in 1903. He resigned because of declining health in 1919 and died the following year.

As a "life and times" effort, Joyce's book has considerable merit, and it is a praiseworthy attempt

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at a legal and political biography. But at the end of the book Griffith still remains something of an enigma. A full biography of Samuel Walker Griffith remains to be written.

F. G. CLARKE
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JOHN YOUNG. *Adventurous Spirits: Australian Migrant Society in Pre-Cession Fiji*. Lawrence, Mass.: University of Queensland Press. 1984. Pp. 417. $37.50.

The cover of John Young's study is colorful and impressive, the book feels solid and is nicely bound, the introductory pages are crisp and clear, but let the reader turn to page 1, chapter 1, and clunk! He discovers that the text looks like a greatly reduced photocopy of a typed thesis. It is all most unattractive and difficult to read, and the footnotes are inconveniently placed at the end of each chapter. Between these covers lies an extremely poor example of word processing and reproduction technology.

As far as content is concerned, Young's largely narrative account traces the increasing presence and influence of European settlers and commerce in Fiji from the slow days of the beachcombers early in the nineteenth century to the phrenetic, short-lived migrant "rush" from Australian colonies and New Zealand in the 1860s and early 1870s. This "rush" was based on the boom in cotton plantations in Fiji in the 1860s, when cotton supplies from the U.S. to Britain and Europe were disrupted by the American Civil War. The book's subtitle mentions only "Australian" migrants (perhaps for marketing reasons), whereas the text makes clear that the settlers, many of whom belonged to the peripatetic gold rush generation, landed from Australia and New Zealand; indeed, an appendix listing a sample of Fiji settlers shows that over one-third came from New Zealand. Young's tale ends with the economic collapse of the cotton industry and the cession of Fiji to Britain in 1873. Although the focus of this tale is on Europeans and their mainly commercial activities, Young places them firmly in the context of Fijian life style and politics, and he explains clearly how economic conditions, together with both indigenous Fijian and colonial political realities, precluded the establishment in Fiji of a large-scale, permanent European "settler society."

Much of this material derives from the author's 1969 doctoral thesis from Adelaide (and not Oxford in 1967, as the cover blurb misleadingly implies). Young has published several articles in the interim that summarize many of the findings of his dissertation, but other writers on Fiji like Deryck Scarr, Caroline Ralston, Peter France, David Routledge, and Evelyn Stokes have long since contributed considerably to the literature on European activity in pre-cession Fiji. Anyone familiar with this material will find this particular book, for all its academic soundness, predictable and even slightly old-fashioned. Young's claim that it throws "new light" on the history of Fiji is just a little hopeful. And this book does not reflect recent international scholarship on the nature of nineteenth-century "white" settler societies on colonial frontiers.

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UNITED STATES

DAVID A. HOLLINGER. *In the American Province: Studies in the History and Historiography of Ideas*. Bloomington: Indiana University Press. 1985. Pp. xi, 222. $22.50.

Not many historians in their forties have written enough important essays to justify collecting and republishing them. David A. Hollinger has. Ten of the eleven essays published in this volume have been published before, and they go as far back as Hollinger's graduate school days. Yet I think even the most demanding critic will feel, on reading this volume, that each of the essays merits reprinting and that each gains significance from being read in company with the others.

The collection possesses more unity than anyone had a right to expect of pieces prepared for different occasions. The first three essays (on William James, Pragmatism, and Walter Lippmann) all reflect Hollinger's abiding concern with the place of science in American culture. Indeed, science or its cultural ramifications so often commands his attention in these and later essays that it could have served as the theme of the collection. But the theme suggested by the book's title, "provincialism," serves equally well. It receives fullest development in the fourth, fifth, and sixth essays, which explore the significance of ethnic diversity and shed light especially on that distinctive impulse to flee from ethnic identity that virtually defines the modern intellectual. Included here is "The Canon and Its Keepers: Modernism and Mid-Twentieth-Century American Intellectuals," previously unpublished, an affectionate deflation of the mystique of modern literature cultivated by literary critics during the 1940s and 1950s.

Studies of intellectuals and their ideas give way to historiographical essays in the second half of the book. My own favorite, "T. S. Kuhn's Theory of Science and Its Implications of History," appears here, as do four other pieces, all devoted to the craft of intellectual history. One examines Perry Miller,

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the field's most enduring practitioner; another explores the implications of defining intellectual history as the reconstruction of collective "discourses"; still another sums up the state of the field today; and there is a short piece on philosopher Richard Rorty, whose radical historicism Hollinger takes to be a good omen for the future of intellectual history.

What is it about these articles that entitles them to the rare distinction of republication? Uncommon intelligence in the first place, and a dense, unusually precise, and self-reflective prose style in the second. Allied with these virtues is the fact that Hollinger stands squarely in the mainstream of his field. No practitioner at work today is more closely attuned to the currents of professional opinion among intellectual historians or more even-handed in his treatment of the issues that divide them. "Nothing in excess" could be Hollinger's motto. He admires Quentin Skinner and Clifford Geertz but finds nice things to say about Arthur Lovejoy; he insists that the ideas and arguments that intellectual historians examine are "social acts," even though their social dimension seldom holds his own attention for long; he endorses cosmopolitanism, even as he waxes eloquent on the strengths of the province. Many historians take greater speculative risks, and some may have stronger claims to originality. There is no one, however, whose work is more erudite, better balanced, or more representative of what the profession, at its collective best, can be.

THOMAS L. HASKELL
Rice University

MORTON BORDEN. *Jews, Turks, and Infidels.* Chapel Hill: University of North Carolina Press. 1984. Pp. x, 163. $17.95.

During the past several years, serious attempts have been made to alter the First Amendment of the Constitution to allow for organized prayer in the public schools. If advocates of the position had succeeded, it would have been the first time the Bill of Rights had been significantly tampered with in two hundred years. We have seen statutory attempts to accomplish the same result. We have seen successful efforts to implement equal access legislation that would require public schools to allow religious clubs to meet on school premises, thus once again dividing children along sectarian lines. And we have seen Supreme Court decisions permitting exclusively Christian symbols to be displayed at public expense and permitting other breaches of the separation between church and state.

As Morton Borden demonstrates in this brief but timely, well-documented, and well-written book, the current controversy is not transpiring in a historical vacuum. There always have been forces in this nation that have worked to Christianize America. This was a battle, initiated even before the revolution, to determine the nature of the American political experiment. In general, Borden indicates, some form of religious restriction was the rule rather than the exception in all colonies. Regrettably, Borden does not provide sufficient historical contextualization for this phenomenon, nor does he indicate the extent of implementation of this legislation. Nevertheless, it is significant that in Virginia, for example, Jews were simply excluded at the outset. Maryland prescribed the death penalty for blasphemy and fines and prison for profanation of the Lord's day. In colonial Pennsylvania, Jews could not legally vote, hold public office, participate fully in business, or hold public worship. Religious tolerance was thus not always the rule in early colonial society.

Borden skillfully details aspects of the Jewish struggle to achieve religious equality during the colonial period and in the century following ratification of the Constitution. Many Americans defined the United States as a Christian nation. Jews and other non-Christians could worship as they pleased but should have no right to participate in government. Some of the same state constitutions that guaranteed religious liberty also contained provisions that restricted officeholding to Christians or to Protestants. In some states, like New Hampshire, North Carolina, and New Jersey, these restrictions were defended and maintained well into the nineteenth century. In addition, in the mid-nineteenth century, organizations like the National Reform Association, led by Supreme Court Justice William Strong from 1867 to 1873, initiated serious efforts on the national level to amend the Constitution to recognize the divinity of Christ and to enforce Sunday laws.

Borden persuasively argues that the expansion of religious liberty was not an inevitable result of the revolution, nor even of the Constitution, and it did not come easily. Full legal and religious equality for "Jews, Turks, and infidels" would come only after a long and protracted struggle. Religious liberty had to be fought for in legislative halls and in state and federal courts. That it was achieved was a result as much of Jewish vigilance, effort, organizational skill, and endurance as of an indigenous spirit of American tolerance. Especially in the nineteenth century, a resurgent and imperial Protestantism attempted to subvert what gains had been achieved. The echoes of that campaign may be reverberating once again.

Borden adds to the understanding of the texture and substance of Jewish-Christian relations in the eighteenth and nineteenth centuries and thereby contributes to an appreciation of the religious origins of American anti-Semitism. Although some may disagree with the rather negative portrait he

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draws, scholars interested in American Jewish history, American anti-Semitism, and Jewish-Christian relations will have to confront seriously the impressive evidence Borden musters and the analysis he provides.

MICHAEL N. DOBKOWSKI
Hobart and William Smith Colleges

KURT R. SPILLMANN. *Amerikas Ideologie des Friedens: Ursprünge, Formwandlungen und geschichtliche Auswirkungen des amerikanischen Glaubens an den Mythos von einer freidlichen Weltordnung*. New York: Peter Lang. 1984. Pp. 341.

Kurt R. Spillmann's study probes the American faith in a universal order based on peace by seeking to identify the origin, modification, and consequences of that ideology during five centuries. The carefully laid-out sections correspond to these formidable tasks. A part of the introduction also provides definitions of "ideology" as an all-encompassing, normative system of thought, of "American" as a specific national characteristic, and of "peace" as a set of expectations about conflict. By design, Spillmann intends neither to make contributions to specific issues in an extensive argument nor to account for the most recent interpretations of them.

The resulting survey, extensively documented with quotations and familiar evidence, forges an understanding of the close ideological and psychic relationship between well-known elements of American history, particularly the body of thought shaping foreign policy. Spillmann identifies the religious expectations of the Puritans of Massachusetts Bay Colony as determining the character of English settlements in seventeenth-century North America, and he then traces the body of Puritan thought back to its Judeo-Christian origins and forward to its erosion in the twentieth century. This tour de force might be described as a review of millennialism as it affects concepts of history, culminating in a Puritan variety that, in its changing forms, imbued generations of Americans with the notion that they occupied a unique place in a historical development oriented toward the winning of peace as a concomitant of the eschatology.

Discussion of the efforts to implement changing forms of the ideology of peace at home and abroad centers on an assessment of the ideas of the revolution and Manifest Destiny as well as the conflicts resulting from the pressure to conform implicit in the envisioned Americanization of Indians, blacks, and immigrants. The sketch of a foreign policy moving from isolation to crusade, with imperialism thrown in for good measure, stresses the role of the ideology of peace in the formation of the League of Nations and the United Nations. He explains the American intervention in Vietnam as the action of a nation transformed into a global power against its will at a moment when parts of the world had grown weary of American ideals.

The author succeeds in identifying the presence of an ideology of peace, based on a rather narrow foundation of political utterances and self-assessments that sound frequently like afterclaps of Puritanism. As a Swiss scholar with a commitment to United States history, Spillmann is addressing readers in Central Europe, but they might gain a greater awareness of the complexity of that ideology in the United States were it not derived exclusively from the Puritanism of Massachusetts Bay Colony. Furthermore, an evaluation of the language of the quoted documents might have broadened the perspective, because insights can be gained not only from what is said but also from how it is said. These suggestions come more readily to mind in light of the author's splendid treatment of reformation theology pertaining to the development of millennialism in America.

GUNTHER BARTH
University of California, Berkeley

BARBARA MILLER SOLOMON. *In the Company of Educated Women: A History of Women and Higher Education in America*. New Haven: Yale University Press. 1985. Pp. xxi, 298. $25.00.

This long-awaited book traces women's pursuit of educational equity over three centuries. Although operationally defined as "opportunities equal to those of men," education equity, Barbara Miller Solomon acknowledges, involves more than institutional access. Today, "in a reversal of history, women have more collegiate options than men" (p. 207). Yet, even as women's opportunities broaden, "women are still the second sex in most of academia" (p. 208), for the coeducational experience has "made plain both directly and indirectly what could be denied at women's colleges, that American society attached greater importance to men's achievements" (p. xix). Widening of access, says Solomon, has not transformed the nature of higher education in which male values structure the disciplines, male traditions dominate campus life and government, and male perspectives organize extracurricular activity.

Solomon draws these conclusions from an analysis that emphasizes the social context of women's struggle for education equity—specifically, the dynamic between demands for equal access to higher education and opposition to these demands. Cultural definitions of women's social role and of female achievement have influenced both the availability of

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higher education for women and its nature. Yet women's pursuit of higher education was not an individual quest; their participation was influenced by private as well as social definitions of female accomplishment and structured by the incentives and opportunities provided (or denied) by family, community, and institution. For many, higher education meant social mobility or professional advancement. But, whether women were inspired by a feminist consciousness and whether their efforts indicated gender loyalty or not, their actions as educated women redefined America's understanding of female accomplishment.

Solomon's study supersedes the two standard works in the field: Thomas Woody's *A History of Women's Education in the U.S.* (1929) and Mabel Newcomer's *A Century of Higher Education for American Women* (1959). Unlike these institutional histories, Solomon emphasizes women's experience of education. The changing parameters of opportunity and aspiration, college life, attitudes toward women's work and marriage, and the strength of feminism are traced from one generation of college women to the next. Although each generation faced a unique set of circumstances, all experienced the same critical dilemma: how, after college, was an educated woman to live up to the promise of her education and yet fulfill her female role?

Solomon gives greatest attention to the generation of 1870–1920, whose participation and achievements in higher education were proportionally greatest. Yet there are new insights throughout—the transformation of academy courses of study into college curricula in the early national period, for example. Solomon's greatest contribution, however, is her synthesis of many institutional and regional histories with women's history. This enables her to compare the various components of women's educational experience (the search for knowledge, the sense of vocation, the development of an identity apart from family) across racial, class, ethnic, and religious lines. Use of memoirs, oral histories, and interviews provide revealing personal anecdotes that, though impressionistic and perhaps more opportunistically than systematically gathered, give individual expression to generational concerns.

This is one of several recent works that, in returning to the study of women's public behavior, herald a transition in the field of women's history. Solomon's achievement is that she does not simply refocus attention from the private to the public sphere but rather underscores the connections between the two. She interprets women's pursuit of education equity as it is influenced by and as it affects public roles and attitudes and as it is shaped by and as it structures personal life.

LEE CHAMBERS-SCHILLER
University of Colorado

PETER DUIGNAN and L. H. GANN. *The United States and Africa: A History*. New York: Cambridge University Press or Hoover Institution, Stanford, Calif. 1984. Pp. xiv, 450. $29.95.

In recent years historians have begun to pay more and more attention to American relations with the Third World. John De Novo and Thomas Bryson, for example, have written about the Middle East, and the present reviewer has published on the North Atlantic islands. Prior to writing their latest volume Peter Duignan and L. H. Gann (along with Peter Collins and Clarence Clendenen) penned three brief monographs during the mid-1960s dealing with various aspects of African history before 1900.

The present book far surpasses the three earlier monographs in scope and sophistication. Since it basically ignores the northern tier of African states, however, it would have been far more accurate had the publishers and authors titled this volume *The United States and Subsaharan Africa: A History*. It thus does not supersede this reviewer's treatment of Morocco, Algeria, Tunisia, Libya, and Egypt in *Clash of Titans* (1974).

Nevertheless, Gann and Duignan's book does cover most of the African continent and does so in a more thorough fashion than any other monograph yet published on African-American relations. Admittedly, such topics as the transatlantic slave trade and African colonization have been the subject of earlier books, but Duignan and Gann have compiled so much data on the activities of both merchants and missionaries that their synthesis goes far beyond earlier treatments. They also examine the role of whalers, explorers, soldiers, miners, hunters, and scientists as well.

So comprehensive, in fact, is their treatment of the minor as well as the major actors on the African scene that their book at times resembles an encyclopedia rather than a monograph. Nevertheless, where there is an intellectual thread to pursue the authors do follow it, as in their excellent chapter "Black Nationalism and the Search for an African Past."

This reviewer's only real criticism of this book is that the treatment of diplomacy is somewhat unbalanced. Duignan and Gann thoroughly cover Liberia—perhaps excessively so—as well as the Congo and Zanzibar. This is not the case with Ethiopia, though; nothing is said about the inception of diplomatic relations after 1900, and the Italo-Ethiopian War is passed over in a page. More also might have been said about early U.S. diplomatic contacts with South Africa (other than the Boer War) and Madagascar. During the nineteenth century, moreover, the United States signed a series of treaties with various African countries that, in some cases,

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included extraterritorial rights. These treaties long formed the basis for American diplomatic relations with the continent, but the authors largely neglect them.

Since American contacts with Africa during World War I focused on Liberia, Duignan and Gann cover these adequately. Yet for World War II they provide only five isolated references to the continent, despite the fact that the *Foreign Relations* volumes for these years are in print and the diplomatic archives are open to researchers. As for the post–World War II era, the authors resort to a discussion of selected topics rather than a comprehensive treatment. They discuss thoroughly the Soviet threat, the Peace Corps, foreign aid, and South Africa, but they almost totally ignore the equally important series of events that transpired in the Congo between 1960 and 1965.

One of the many positive features of this book is the absence of typographical errors throughout the text. There also are a number of pictures, fifty pages of footnotes, a selected bibliography (which should have included articles as well as books), and a twenty-page, double-column index. Unfortunately, the type is a bit small.

In conclusion, despite the less than thorough diplomatic coverage, this remains a significant book, comprehensive in scope and balanced in treatment. In an era in which many individuals are less concerned about mass slaughter in Afghanistan and Cambodia than about *apartheid* in South Africa, the scholar attempting to be objective about the Third World does so at great risk. Duignan and Gann are to be congratulated for not writing a propaganda tract.

EDWARD W. CHESTER
University of Texas, Arlington

PATRICIA CALDWELL. *The Puritan Conversion Narrative: The Beginnings of American Expression.* (Cambridge Series in American Literature and Culture.) New York: Cambridge University Press. 1983. Pp. x, 210. $19.95.

This volume examines the conversion narrative in Puritan gathered churches as an expressive form and as a primal instance of American expression. Its principal source is fifty-one "relations" by candidates for church membership at Cambridge, Massachusetts, during 1637–44, and two collections, from 1653, of relations from Independant congregations in Dublin and London. The study begins with the narrative of one Elizabeth White (d. 1669) as a type-case of the form's English variant. Caldwell investigates the origin of public conversion narratives, which she derives from the practice in some Puritan congregations of hearing personal confessions of repentance from sin. Her primary concern is the form, content, and tone of narratives from England and New England. Caldwell finds the former to be fluent, conventional, rich in biblical allusions, and complete in describing attainment of grace. The Massachusetts narratives, although manifesting earnestness and emotional intensity, and incorporating biblical frames of reference directly, nonetheless appear almost uniformly groping, troubled, and fragmentary, failing to conclude in joyous assurance. Their focal event is migration to New England, which occasioned spiritual uncertainty and disappointed hopes for the effectiveness of church ordinances and enjoyment of religious feelings in the new land. Metaphors of collective, geographical circumstance dominate. The significance of being in America is integral to understanding personal spiritual condition, and, Caldwell argues, characteristic elements of the American hermeneutical predicament are thus present.

The study's underlying subject is the social and psychological phenomena occasioned by transplanting a semisectarian group from a situation of relative alienation amid familiar institutions to one of practical dominance where institutions must be created afresh. Caldwell's exposition invokes psychological and sociological categories extensively, though not with methodological self-consciousness; the controlling perspective is literary. Consideration of the origin of public narratives underestimates Puritan concern about the substance of the true church and underestimates the conversion narrative as a relation of "experienced" faith that was not reducible to "confession of sin." It seems methodologically risky to juxtapose unedited notes of exchanges in congregational meeting, when the practice was novel, and later, published, narratives, when the form had become an established edificatory genre. Limited acquaintance with the content of Puritan practical theology issues in over- and underinterpretation of the texts.

Nonetheless, Caldwell's characterization of English and American narrative is broadly sustained by the material. Her work qualifies conventional images of Puritan New England as boundlessly confident of divine election: some Puritans, clearly resembled other first-generation immigrants who sought America as promised land and found disappointment and puzzlement. The study identifies an early instance of the "problem of place" in American self-consciousness and points, though indirectly, to the crucial place of verbal forms in Protestant, and especially Puritan, piety.

WILLIAM K. B. STOEVER
Western Washington University

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DAVID S. LOVEJOY. *Religious Enthusiasm in the New World: Heresy to Revolution*. Cambridge: Harvard University Press. 1985. Pp. xiii, 291. $25.00.

Many American colonials did not hold such orthodox faiths as Puritanism and Anglicanism dear, and, although most dissenting groups have been individually studied by scholars, no synthesis of these enthusiasts has been attempted until David S. Lovejoy's recent admirable effort. Enthusiasts were a motley lot, according to Lovejoy, but all of them claimed a "close, warm, emotional relationship with God," one often stamped with anti-intellectualism as well (p. 1). Quickly and incisively, the author describes the roots of enthusiasm in Europe, the often imprecise and enigmatic characteristics of each group, and the impact each had on colonial ideas. Lovejoy is thorough in his catalogue, dealing with sects large and small: Millennialists, Fifth Monarchists, Anabaptists, Familists, Separatists, Donatists, Antinomians, Gortonians, Quakers, Ranters, Mennonites, Labadists, Rosicrucians, Moravians, French Prophets, and New Lights. He covers virtually every group that could remotely be called enthusiastic and provides important information on activities among several Indian tribes and blacks. Lovejoy's contention that enthusiasts played a significant role in early American history is well demonstrated.

This book is lively in style: "Scratch an intense religious belief and you discover tensions," Lovejoy affirms, as he punctuates scholarly quotations with comments about Puritans wishing to "string up" Samuel Gorton and Amsterdam Anabaptists "streaking through the streets of that city" (pp. 62, 96, 125). Lovejoy's study is also soundly researched. He has read virtually everything written by an enthusiast. (The only works I am aware of that he did not find are Samuel Gorton's theological tracts published in England.) His command of the secondary literature is less impressive but still adequate.

Lovejoy's study is not without flaws. The author devotes a disproportionate amount of material to the seventeenth century, excludes detail on the social aspects of communitarian settlements such as the Mennonites and Moravians, mentions the Montanists without explaining who they were, and fails to treat native preachers among the Narragansetts and blacks. These are not matters of great significance, but Lovejoy's contention in his last chapter that, with the revolution, religious enthusiasm metamorphosed into political enthusiasm is more serious. He supplies no direct evidence to link the two forms of radicalism and mentions Baptists as the only revolutionaries who could be termed religious enthusiasts. (He furthermore makes clear that all Baptists did not support the American cause.) Lovejoy's assertion that "enthusiasm traditionally had included a political dimension" (p. 222) is certainly true, but his examples are all prior to 1692. Criticism of "brain-sick enthusiasts" voiced by Tories (p. 224) is irrelevant, because orthodoxy always links together groups that challenge it, whether or not they are actual bedfellows. Still, Lovejoy's overexuberance can be forgiven, for his work is a comprehensive, valuable, and extremely interesting study of a little-examined phenomenon.

LYLE KOEHLER
University of Cincinnati

RICHARD L. BUSHMAN. *King and People in Provincial Massachusetts*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1985. Pp. 280. $25.00.

At least three essays now in print inquire into the political culture of colonial Americans in an attempt to find the origins of the dramatic break with British political culture in the years 1763–76. One, Bernard Bailyn's *Origins of American Politics* (1968), is a rather tortured elaboration of some of the sources of that colonial paranoia vis-à-vis British politics that was eventually to blossom into revolution. Another, so new that I have not yet seen it, is Melvin Yazawa's *From Colonies to Commonwealth*, published by Johns Hopkins late in 1985. Yazawa appears from all descriptions to look beneath conventional political culture into the "familial paradigm" of colonial politics, whose degeneration presumably allowed a revolutionary republican model to take its place. Richard L. Bushman's new book is a third study of our eighteenth-century political culture, nominally on Massachusetts alone but written with an eye to wider generalizations.

Bushman shares with Bailyn the conviction that "there is insufficient evidence of a crisis in colonial society" to merit consideration of the relationship of social change to the origins of the political revolution (p. 245). This stunning ability to ignore the convictions of the participants themselves, as presented in works by Gordon Wood and by Rod Berthoff and John Murrin, to name just a few, should not be held too much against Bushman. For, unlike Bailyn's book, Bushman's is a deep, calm, and reflective essay on the nature of the political culture associated with the imperial relationship in the eighteenth century, which ultimately returns us to the nature of American society regardless of Bushman's failure to find an appropriate social "crisis" that he can relate to the revolution.

Essentially, his book is a long, rich, ruminative explanation of why, in the largest sense, "the Revolution was fought not only to achieve independence from Great Britain but to preserve independence in America" (p. 248). The "independence" that colo-

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nial Massachusetts, at least, already experienced before the revolution included the economic and political independence of its voters and of many of its politicians from the chains of dependency relationships that, in Britain, bound most men to one another in a vast hierarchy culminating in royal placeholders and in the king himself. Using a wealth of profoundly textured case studies drawn from the annals of Massachusetts politics, Bushman suggests that Americans were never truly dependent on this articulated system of influence and patronage. Consequently, they saw only "corruption" and selfish "interest" in the royal officeholders forced on them by an imperial administration and by a Parliament ever more determined to make Americans pay their way within the imperial system. The colonists neither needed nor were likely to get these offices, nor did they benefit much from the patronage networks. To Americans the abstract symbol of "the Crown," to which they were loyal, was not legitimately embodied in the horde of officials sent "to eat out their substance." By 1775 the colonists were ready to hear Thomas Paine tell them that a people could do without a king.

Bushman is in one sense retelling an old story, but he does so most often with a tone, a depth, and a detail that makes his generalizations convincing. Truly, in a dozen senses, independent Americans in Massachusetts were loyal to what was only the shell of what in Britain was conveyed by "the King" or "the Crown." They had adopted these symbols, but not the system of patronage and dependency by which they were operationalized. Faced with having that system externally imposed on them, they rebelled. Bushman's only oversight is that, in seeing no "social crisis" behind this rebellion, he misses the ample evidence that revolutionary Americans in all walks of life were aware of the social trends of the eighteenth century and feared that they were moving toward an indigenous system of scarcity, hierarchy, patronage, dependency, and even "lordships" in some form. Whether they could preserve their history from such a fate or not, many certainly rebelled at having the social trends of their time sanctioned by British policy. Others accepted America's evident fate but resented having the emerging indigenous patronage system absorbed into the vastly more powerful British system. Americans removed British policy by means of a revolution and, under an optimistic Thomas Jefferson and a more pessimistic James Madison, tried for generations more to preserve America's social innocence.

KENNETH A. LOCKRIDGE
University of Michigan

JACKSON TURNER MAIN. *Society and Economy in Colonial Connecticut.* Princeton: Princeton University Press. 1985. Pp. xv, 395. $34.00.

Jackson Turner Main, a distinguished historian of early America, has provided the most detailed and statistically complex study of the social structure of an American colony that we are likely to have for many years. Main's purpose is to study the acquisition and distribution of property in order to measure the degree of opportunity for material success in Connecticut. Two chapters survey the distribution of property in the seventeenth and eighteenth centuries; four others focus on property acquisition among laborers, farmers, craftsmen, professionals, and traders. Main concludes that Connecticut was "a land of plenty for some and of sufficiency for most people" (p. 382). Furthermore, he rejects the prevailing ideas that rising population pressed hard on available land resources in the prerevolutionary decades, that property became less evenly distributed, and that a subsistence farming economy gave way to commercial agriculture. After an early period of difficulty, he argues, conditions underwent periodic ups and downs but no fundamental change. If Robert E. Brown were alive today, he would exult at these findings, for the mountain of data Main has extracted, primarily from probate records and tax lists, is interpreted to show that Connecticut was nearly the middle-class democratic paradise that Brown, in a pathbreaking and controversial book on Massachusetts, described thirty years ago.

The most striking feature of this study is the emphasis on the life cycle and how wealth, or lack of it, correlates closely with age. Poor men, Main argues, were mostly young men on the way to better things. This argument works best for rural areas, where livelihoods depended on land, the acquisition of which was usually delayed until late in the father's life or after his death. Main does not test the life cycle analysis among artisans in towns, where his conclusions are unlikely to hold up.

It will take the most skilled quantitative historians more than a summer's day to evaluate the massive statistical manipulations that crowd this book; certainly it is beyond the ken of this reviewer. Whereas Main included 11 tables in his pioneering *Social Structure of Revolutionary America* (1965), this study displays 147 tables. But for all the statistical fireworks, which give the impression of scientific precision, the data is susceptible of various interpretations. Main, for example, celebrates the fact that two-thirds of married men in their twenties lived above the poverty line, but some may be more impressed that one-third lived below it. He argues that no real changes occurred in social structure between 1700 and 1775, yet his data show that one-tenth of all men were impoverished at the beginning of the century and one-fifth by the 1770s (table 4.11).

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Two weaknesses mar the book—one methodological, one conceptual. Methodologically, the problem lies in Main's failure to adjust income and estate values for price inflation in the eighteenth century; this distorts the data in favor of a more optimistic interpretation of longitudinal change, although even without price adjustments Main's data shows that between 1700 and 1774 there was an overall "decline in the value of men's holdings in property" (p. 125). Conceptually, the entire mode of analysis is useless for advancing an understanding of how the conditions of men's lives affected their political and social outlook because Main deals only with aggregate data, lumping together people from new frontier towns and old settled towns. Hence, we are never permitted to see the secular changes experienced by particular families in particular places. Real people do not react to changes described by statistically blended data for large regions; they act with reference to conditions within their own communities. Regrettably, Main never analyzes how opportunity changed within a single community. Thus, we are left with that most unsatisfying kind of social history—a massive array of data that does little to promote an understanding of aspiration and outlook, of political consciousness and political behavior.

GARY B. NASH
University of California, Los Angeles

THOMAS J. DAVIS. *A Rumor of Revolt: The "Great Negro Plot" in Colonial New York*. New York: Free Press of Macmillan. 1985. Pp. xv, 320. $19.95.

New York City in 1741 extended no further north than Washington Square and included about 2,000 blacks in a population of 11,000. After a cluster of burglaries and fires amid fears of war with France and Spain, the traditional Catholic foes, officials jailed about 200 persons, mostly slaves, and later executed 34 (including 4 whites) and banished 72. The trials—choreographed by Daniel Horsmanden, an ambitious judge and nonslaveholder who believed that slavery endangered society—ended with the prosecution of a white preacher, John Ury, suspected of being a Catholic priest. The conviction of Ury and a handful of black Spanish sailors, helped create a self-fulfilling prophecy that made a frightening conspiracy, "the Great Negro Plot," out of little more than lower-class dram-shop camaraderie, mock rites (with circles, baptisms, and punch bowls), and a few slave thieves and their white fences.

Thomas J. Davis argues that, although scholars are "generally" skeptical about slave conspiracies, which are "often" attributed to "signs of paranoia," the 1741 plot should be seen as an instance of serious slave unrest (p. xi). Although "chances for blacks to overthrow the American slave system were never great," the plot demonstrates "beyond question that slaves in New York City during 1741 clearly talked of doing damage to the society enslaving them" (pp. xi–xii).

The surviving sources that record the event are unusually full and rich with interpretive possibilities: Davis mentions slave plots or actual rebellions in New York (1712), South Carolina (1739), Antigua (1736), and Virginia (1800); many footnotes call attention to extensive background literature concerning such structural conditions as the education of blacks, imprisonment for debt, prices, the laboring situation, and xenophobia. Even a community study could be constructed on the basis of these sources: "A portrait of early New York," Davis writes, "emerged in the detail of the slaves' life and work—fetching water from communal wells . . . carrying footwarmers . . . or seeking their own comforts in the free time they had" (p. x).

But little of this intrudes into a text whose purpose is to provide "a straightforward, readable account" (p. xiii) consisting mainly of the interrogations and trials of slaves set in a painstakingly detailed chronology of four months. The narrative, which is often rendered as bursts of conversations by desperate people attempting to save themselves by naming names, may replicate the fragmented reality of officials as they tried to make a coherent case out of anti-black and anti-Catholic phobias, but it does so at the expense of an analysis of character and structure. *Rumor of Revolt*, however, is a useful beginning for understanding the world of urban slaves in Britain's northern mainland colonies.

MICHAEL MULLIN
California State University, Sacramento

JOHN S. PANCAKE. *This Destructive War: The British Campaign in the Carolinas, 1780–1782*. University: University of Alabama Press. 1985. Pp. xv, 293. $27.50.

After publishing his memorable *1777: The Year of the Hangman* (1977), covering the Burgoyne and Howe campaign, John S. Pancake turned his attention to the last three years of the war in the Carolinas. "We military historians," he readily admits, "are labeled by some of our condescending colleagues as the 'drum and bugle corps,' and it would be less than honest to say that we are not gripped by the drama and panoply of the battlefield" (p. xiii). This fascination he transfers to the reader, who will learn that in the South the war was much more a civil war and

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often bitter, vengeful, and bloody. Glorious it was not.

The first five years of the revolution are not reviewed except as necessary to define the changing problem for the British and to introduce their commanders and Lord Germain, American secretary. The contrasting viewpoints among the British are examined: all of them wished to Americanize the war at least expense, but Howe, Clinton, and Cornwallis quickly learned the absurdity of Germain's naive faith that the Loyalists would fight the rebels for them if British regulars pointed the way. For all of the British it remained a war of "posts," or places to be seized that would force the rebels to surrender, but the colonials did not know that. Thinking it unprofessional, no Englishman faced the bitter truth that, as long as Washington maintained an army in the field, the revolution would continue. The "show of force" was the emperor's new clothes.

The British failed ignominiously at Charleston in 1776 and only partially redeemed themselves by seizing Georgia in 1779. The stage was then set for opening a southern theater of war. Pancake pauses to review the various military antagonists and their training, then focuses on the siege of Charleston in 1780. The defense left much to be desired, and, when the victorious Clinton departed for New York in June, the British seemed secure in their claim to a second southern colony. The defeat of Gates at Camden in August confirmed British control, and Loyalists rallied eagerly under royal banners.

But events overcame the British advantage. Cornwallis was left in command and revealed extraordinary stupidity. Southern partisan leaders like Marion, Sumter, and Pickens emerged to disrupt communications, engage detachments, and harry the Loyalists. Greene replaced Gates as commander in the South and showed a grasp of the situation that Cornwallis never displayed. These were the factors, Pancake makes clear, that gradually reversed the positions of the adversaries and led to British defeat.

It is a dramatic story, and the author knows how to organize his narrative for enchanting effect. Despite an occasional trite metaphor, he writes very well; the book is both exciting and accurate and will arouse the jaded undergraduate's lasting interest in American history. Scholars will find Pancake's interpretations logical and penetrating. He has used all the right manuscript collections, the eyewitness printed sources, and the best secondary accounts. His extensive footnotes are gathered at the back of the book. Scattered throughout are a dozen maps and portraits of thirteen commanders. The University of Alabama Press has designed a handsome volume.

HOWARD H. PECKHAM
EMERITUS
University of Michigan

STEVEN R. BOYD, editor. *The Whiskey Rebellion: Past and Present Perspectives.* (Contributions in American History, number 109.) Westport, Conn.: Greenwood. 1985. Pp. xii, 212. $35.00.

As this volume of essays and edited documents makes clear, the Whiskey Rebellion of 1794 is one of the episodes in early American history that has benefited greatly from the increasing propensity of historians to research their subjects "from the bottom up." The excellent historiographical survey by Thomas P. Slaughter shows that historians are beginning to get beyond the partisan battle between the "friends of liberty" who defended the rebellion (contemporaries like Hugh Henry Brackenridge and William Findley, whose accounts are printed in this volume, and such later historians as Leland Baldwin) and "friends of order" who praised the vigorous federal suppression of the rebels (Alexander Hamilton and Judge Alexander Addison, whose contemporary assessments also are printed here, and such modern historians as Richard H. Kohn and Jacob E. Cooke). The original essays published in the last half of this book confirm Slaughter's point; recent studies of formerly obscure areas, people, and events clearly illustrate the depth and complexity of the motives and actions on both sides of the Whiskey Rebellion.

Mary K. Bonsteel Tachau's contribution, drawn largely from her book on the federal courts in early Kentucky, shows that resistance to the whiskey excise tax was as pervasive in Kentucky as in Pennsylvania, though more passively expressed. James Roger Sharp traces the effect of the rebellion in Pennsylvania on the debate over the legitimacy of violent resistance to government when peaceful democratic recourses are available. Roland M. Baumann shows that manufacturers in Philadelphia were as opposed to excise taxes as the farmers who produced whiskey, and, although the manufacturers made no contribution to the rebellion, they did desert the Federalist party in response to Hamilton's tariff and excise policy. Finally, Richard A. Ifft demonstrates that competition between federal and state judiciaries played a part in both the instigation and the punishment of the rebellion.

Unfortunately, each of these original essays deals with only a limited aspect of the rebellion. Despite a valiant attempt by the editor, Steven R. Boyd, the book never quite comes together. Some of the most important new information from recent publications that are not printed in this volume can only be introduced in sidelong glances. This includes the findings of Jacob Cooke's Namierist dissection of the politics of western Pennsylvania and Dodee Fennel's fascinating analysis of the actual economic impact of the excise on the farmers. The portrait that emerges is thus somewhat fractured, out of proportion, and

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beleaguered by repetitions of authors who did not know what the others were writing. But this problem is inevitable in any such edited collection, and the original essays and historiographical summary are worth enduring this shortcoming.

JERALD A. COMBS
San Francisco State University

SILVIO A. BEDINI. *Thomas Jefferson and His Copying Machines.* (Monticello Monograph Series.) Charlottesville: University Press of Virginia. 1984. Pp. xvi, 239. $22.50.

Can anyone remember how we copied letters before Xerox? For Thomas Jefferson the problem was even more severe. Silvio A. Bedini's book tells us how Jefferson overcame the difficulty of making copies during the quill-pen era without relying on a copyist: he bought copying presses of the sort patented by James Watt in 1780 and, with clearer and more durable results, various models of the writing machine patented in 1803 by English-born inventor John Isaac Hawkins and manufactured by his friend Charles Willson Peale, the Philadelphian artist. Dubbed the "polygraph," it was not a lie detector but a mechanical apparatus whereby one or more extra quill or steel pens simultaneously copied on separate sheets of paper whatever the writer was writing with the first pen.

Jefferson did not invent "his" copying machines, then, but bought and used them for many years. He did, however, contribute to their development by suggesting changes—mostly for reducing their size—to Peale, who customized his manufacture accordingly. Jefferson also praised the polygraph machines to his friends, endorsed them in Peale's advertisements, and sent them as gifts. Bedini has located a prodigious number of letters about the polygraph that were exchanged between Jefferson, Peale, and others, including Hawkins and the architect-engineer Benjamin Henry Latrobe, who was another enthusiastic long-term user of the machine. That these letters survive is in large part because of the copying machine itself (by which they were written), for, as Jefferson astutely observed about keeping documents safe, "not . . . vaults and locks which fence them from the public eye and use . . . but . . . multiplication of copies . . . shall place them beyond the reach of accident" (p. 3).

Bedini has shaped his material into a gracefully straightforward narrative about a narrow topic. In his first and last chapters he sets "Jefferson's" copying machines into the historical stream of copying devices from Christoph Scheiner's pantograph of 1631 to the twentieth-century invention of xerography and of signature machines, but the middle seven chapters concentrate on the development of those machines that Jefferson and his friends bought, particularly the Hawkins-Peale polygraph. Readers may encounter stretches of too many details, but overall they give a fresh impression of the famous men in Jefferson's international circle of acquaintances as they dickered and tinkered with an intriguing device. By 1804 the polygraph was so easy to use that Peale was able to advertise, "on the first trial the feeblest female hand, to her astonishment, produces two original copies" (p. 99).

The polygraph did not succeed commercially, however, for it required frequent repair and Peale's manufacturing costs remained about twice its selling price of fifty to sixty dollars. Nevertheless, he kept altering its design and custom-building special units for Jefferson. In addition to Jefferson buffs, this case study should appeal to students of the history of mechanism, preindustrial manufacturing enterprises, and mores of advertising, patenting, and gift giving.

CAROLYN C. COOPER
National Museum of American History

R. KENT NEWMYER. *Supreme Court Justice Joseph Story: Statesman of the Old Republic.* (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1985. Pp. xvii, 490. $33.00.

Joseph Story (1779–1845)—politician, justice of the United States Supreme Court, professor at the Harvard Law School, and author of treatises (amounting to some thousands of pages) on the Constitution, equity, and various aspects of commercial law—is an impressive but somewhat forbidding figure in American legal history. R. Kent Newmyer's book is the latest and most comprehensive study of Story's life and career. The book stresses the career rather than the life; once Story gets to the United States Supreme Court, Newmyer seems to lose interest in Story the person and devotes himself almost entirely to Story the judge, the author, and the professor. Nonetheless, this is on the whole the most detailed account we have of Story, and it is likely to remain definitive.

Story was an actor in numerous great legal dramas, many of which have been explored in detail by other scholars; it is no criticism of Newmyer that he does not add very much to what is known about the Charles River bridge case or about the Marshall and Taney courts in general. But Newmyer handles some of the major cases—notably, *Swift* v. *Tyson*—in exemplary fashion and presents new material on some fields (the admiralty, for example) that have not engaged scholars to the same extent. The Harvard years, too, are treated in a fresh and illuminating way.

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A great deal of patient work went into this book, for which students of legal and political history have good cause to be grateful. Newmyer apparently came down with quite a case of hero worship; he admires Story inordinately as a person, a judge, a teacher, and an author. This last is quite a feat, since Story was (by modern tastes) quite turgid and pedantic. Occasionally, Newmyer's admiration seems to get in the way of rational assessment; perhaps this is an occupational hazard of biographers.

Newmyer avoids a far worse occupational hazard of legal historians. He is measured, skeptical, and sensible about the role of law, lawyers, and legal phenomena in molding the events of American history. He describes, in convincing detail, Story's ideology, republican faith, devotion to "legal science," and ambitious desire to use bench and classroom as vehicles for affecting policy. But Newmyer does not take these at face value. Indeed, when he asks himself about Story's long-term influence on American history, he comes up with a rather modest assessment. Newmyer does not believe that American lawyers, including Story and his Harvard students, had the capacity to "control" American society; rather, they were "instruments" of society's "radical transformation" and were "carried along by the forces they worked to unleash" (pp. 269–70). In general, Newmyer takes a careful and commonsense view of the relationship between Story's work and the currents of thought and event in the outside society. This is one of the greatest strengths of this admirable book.

LAWRENCE M. FRIEDMAN
Stanford University

STEPHEN W. BROWN. *Voice of the New West: John G. Jackson, His Life and Times.* Macon, Ga.: Mercer University Press. 1985. Pp. xxv, 262. $22.95.

In the foreword John G. Jackson is introduced as a "deserving but not Olympian" statesman, who is contrasted with the "towering giants" of America's political history. In the preface he is touted as one of the "American Leaders of secondary rank whose lives and careers elucidate the nation's history and add dimension to the broad outlines shaped by the great figures who towered above them" (p. xi). Regrettably, Jackson's "secondary rank" shines forth more luminously from Stephen W. Brown's book than does his "undeserved obscurity."

Jackson, a disciple of James Madison (his brother-in-law) and of Thomas Jefferson, represented western Virginia in the state legislature and in the U.S. House of Representatives; later he served as a federal district judge. Most interesting is his career as the major industrial entrepreneur of what would become West Virginia. Because so many of his achievements—as distinguished from his efforts—were associated with his salt and iron industries, his woolen and tanning establishments, his bank, and his interest in improving transportation to and through the area, it is regrettable that Brown does not provide a map to clarify his descriptions of Jackson's "New West."

Jackson represents a transition from Jeffersonian Republicanism to Henry Clay's American system. As a western entrepreneur, he vigorously supported the National Bank, the protective tariff, and, especially, internal improvements at national expense. He remained, however, enough of an Old Republican continually to advance unsuccessful constitutional amendments designed to legitimize federal action in these areas. Jackson was in the forefront of a number of important political issues of his day, but he was so far front that he seldom had the satisfaction of seeing his bills succeed. Jackson was also an early and unsuccessful advocate of the democratic reform of the Virginia constitution.

We now know all we are ever likely to know about Jackson. Brown wrote both master's thesis and doctoral dissertation on Jackson, and in its propensity to tell all his book is like a dissertation. An example is Brown's reference to Jackson's need "to form opinions and respond to scores of routine matters" (p. 176), followed by a recitation of those routine opinions and responses; most readers would willingly forego the latter. Brown even gives two separate accounts of Jackson's jaunt to Georgetown to purchase $367 worth of silverware for his Clarksburg home (pp. 138, 146). Enough is enough!

DONALD O. DEWEY
California State University, Los Angeles

STEVEN J. ROSS. *Workers on the Edge: Work, Leisure, and Politics in Industrializing Cincinnati, 1788–1890.* (Columbia History of Urban Life.) New York: Columbia University Press. 1985. Pp. xx, 406. $35.00.

This excellent book merits special attention. Without question it is a major contribution to the continuing reinterpretation of American working-class history. One reason is that Steven J. Ross looks closely at a major inland city, shifting attention away from the oft-studied seaboard ports and factory towns. More important, Ross covers a long period of Cincinnati's social and labor history. Apart from Alan Dawley's *Class and Community* (1976), no recent local study of nineteenth-century American workers has been so ambitious or done more to confront the problems of periodization and interpretation that still perplex the field.

Ross divides his story into three phases that correspond both to long swings in the American busi-

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ness cycle and to Marx's ideal stages of industrial capitalist development. During the "age of the artisan" (1788–1843), Cincinnati's craft workers faced many of the structural changes in wage relations evident in the larger seaboard cities. Like their eastern counterparts, they formed parties and unions and "developed a language and mode of analysis that merged traditional tenets of the producer ideology with the vaunted principles of republican ideology" (p. 45). Ross contends this analysis led workers "to misread the roots of modern economic crises" (p. 61). Yet it also provided its adherents with "a metaphor and image of a more perfect world" useful in political campaigns and strikes (pp. 61–62).

The republican image persisted, with some alterations, into the "age of manufacturing" (1843–73) and the "age of modern industry" (1873–90). In one of his best chapters, Ross details the uneven changes in workplace authority as they affected a wide range of Cincinnatians after 1840. This myriad of experiences, he argues, divided workers according to sex, skill, ethnicity, race, and religion but also gave them all an "activity and role"—as wage earners—around which they could unite.

Against this backdrop, Ross offers a beautifully researched, ironic account of local unionism, electoral politics, and working-class leisure from the Age of Jackson onward. Most of the time, working-class republicanism helped propel protest movements, but it also effectively separated the economic sphere from the workers' political and social lives. Cincinnati workers remained militant on economic issues, but they proved unwilling to support workingmen's political insurgencies. They also stayed divided over "social" issues like temperance.

Nevertheless, a combination of rapid mechanization and prolonged depression did lead to a crescendo of working-class unrest and political activity after 1873. A fiery courthouse riot in 1884, followed two years later by an impressive series of May Day strikes, frightened elite Cincinnati into believing that the Paris Commune had finally come to Ohio. Finally, in 1887, the mainly working-class United Labor party (ULP) gained one-third of the city's contested city council seats and came within a whisker of capturing the mayoralty. Soon thereafter the ULP disintegrated, the victim of internal dissension.

What makes this story so compelling (apart from Ross's scholarly craftsmanship) is that it so closely parallels the story, *mutatis mutandis*, in other northern commercial-industrial cities. Systematic comparisons with other places would have strengthened the book; nevertheless, Ross accurately describes, in the context of one locale, a general process that swept through industrializing urban America, more or less in accord with his synopsis. Consequently, *Workers on the Edge* might be taken as a rough blueprint for the long-awaited synthesis of the "new" labor history, one that may help establish the Gilded Age—and the emergence of what Herbert Gutman called the "second American working class"—as the most critical phase in nineteenth-century working-class history.

To be sure, this book is not definitive, even on its own terms. At times, an idealist, deterministic quality pervades its treatment of working-class republicanism, notably, when Ross ascribes working-class reluctance to break with mainstream parties to republicanism itself. Ross might have reflected more generally on the themes of class consciousness and politics in his crucial final chapter on the ULP; as is, that chapter, and the book, break off rather abruptly. Vital dimensions of working-class life and politics—especially those surrounding gender and sexuality—are neglected here, as in other recent local studies of labor.

Debating and clarifying these matters, however, should help American social historians move beyond rebutting consensus history and toward a better understanding of the politics of nineteenth-century working-class history. As they do so, they will be greatly indebted to this book and its author for having pushed the discussion, and the standards of research and writing, to such a high level.

SEAN WILENTZ
Princeton University

PRISCILLA FERGUSON CLEMENT. *Welfare and the Poor in the Nineteenth-Century City: Philadelphia, 1800–1854.* Cranberry, N.J.: Associated University Presses, for Fairleigh Dickinson, Rutherford, N.J. 1985. Pp. 223. $27.50.

This book adds to existing knowledge of early nineteenth-century American social welfare history. Priscilla Ferguson Clement traces the dimensions of public and private welfare policy during a period of rapid urban growth. During the first half of the century, Philadelphia's population increased fivefold, rising from about 61,500 in 1800 to over 300,000 in 1850. While confronting the problems of growth, the city also suffered periodic economic depressions stemming from the embargo of 1807, the panic of 1819, and the panic of 1837. Social diversity resulting from immigration undermined the city's traditional sense of community and contributed to shifting attitudes toward the poor.

The book begins with a chapter on the poor, telling us who they were and why they needed assistance. The rest of the book describes and analyzes the city's welfare structure. Building on the British experience, especially the Elizabethan Poor Law of 1601, colonial Americans established an elaborate system of poor relief. By the early nine-

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teenth century, the system in Philadelphia and other cities was composed of two essential elements: outdoor relief, or cash payments and other assistance to those temporarily in need, and the almshouse for those whose dependency was more permanent. Debates over the merits of outdoor relief and institutionalization remained at the heart of welfare policy throughout the period. In addition, private charity proliferated as the city grew, and the needs of various categories of dependents—widows, children, immigrants, blacks—became apparent. Charitable societies provided the poor with medical aid, temporary shelter, soup, fuel, work, and Bibles. The final chapter attempts to draw some parallels between the early Philadelphia experience and the current welfare problem.

Clement's narrative also addresses changing attitudes toward the poor and the impact of such attitudinal shifts on policy. Her chief interpretive argument is that welfare policy is cyclical, depending primarily on the state of the economy. In the case of Philadelphia, economic depression after 1819 provoked a harsh attitude toward dependency and resulted in welfare "reform" in the late 1820s. The new policy cut back outdoor cash aid and, partially as a deterrent, required institutionalization of those seeking aid. Another key interpretive idea carried throughout the book relates to the changing pattern of motivation behind Philadelphia's social welfare programs. As Clement puts it, "the humanitarian spirit characteristic of the relatively prosperous years in the early nineteenth century gave way to a greater interest in social control and thrift during and after the depression following the War of 1812" (p. 118).

Clement makes a persuasive case for the shift away from benevolent motivations, but her use of the concept of social control is particularly weak and oversimplified. The author uses the term often but never clearly explains what it means or fully elaborates the mechanisms by which it was implemented. Another weakness is the failure to explore the links between Philadelphia welfare leaders and European charity reformers, whose ideas were widely adopted in American cities by the 1820s. Examination of the transatlantic reform dimension might have provided an alternative explanation for the welfare changes that Clement lays to economic depression. Despite these interpretive lapses, this exhaustively researched book makes a useful contribution to the history of urban social welfare.

RAYMOND A. MOHL
Florida Atlantic University

BARBARA JEANNE FIELDS. *Slavery and Freedom on the Middle Ground: Maryland during the Nineteenth Century.* (Yale Historical Publications, Miscellany, number 123.) New Haven: Yale University Press. 1985. Pp. xv, 268. $27.50.

Barbara Jeanne Fields has perhaps already attained an enviable reputation for wit, eloquence, and substance among those in the historical profession who have followed her progress. This perceptive and provocative book adds luster to her reputation. Fields pictures Maryland as a battleground on which the political economy of slave labor confronted the political economy of free labor, resulting in the latter's ultimate success. It was a society in transition from a premodern or precapitalistic economic and social structure toward that of mature capitalism. Civil war accelerated but did not determine the process, for slavery was moribund long before that cataclysm. Although the free economy of antebellum northern Maryland was the most vital aspect of the state's economic structure, it was held politically subject to the land- and slaveowning elite of southern Maryland and the Eastern Shore. Baltimore, the product of the northeastern grain trade and one of the most vibrant cities in the slave South, was nevertheless peripheral to the peculiar institution that grounded southern society, for the economic transition of which the city was a part made slavery at least partially undesirable. Maryland thus formed a "middle ground" that united conflicting economic systems. Its governing political elite could separate neither from the more vigorous northern part of the state nor from the South, which supported the institutional basis of its security.

But Fields rejects this "middle ground" as the locus of virtue in a society rent by extremists. Rather, she views Maryland as a social hybrid pulled in opposite directions by the force of circumstances. The state's politicians took an intermediate stance not because they were wiser but because they objected to making an ultimately obligatory choice. Of course, perceiving the attempt to save both the Union and slavery as misguided, if not ignoble, is a philosophical outlook that one would like to regard as the unquestioned modern assumption of an enlightened age, but neither some recent scholarship nor the current political climate suggests that that is the case.

Fields effectively demonstrates shortcomings in neoclassical econometric historical analysis. She is devastating, for example, on the question of slavery's compatibility with industrial development and urban growth. Slavery declined everywhere in the urban South, and those areas in which slaves were used in industry were exceptions to a general rule. She makes a clear distinction between slavery as an individual attribute of the laborer and slavery as an organizing principle of labor and society. That the slave can function economically and effectively does not argue for the effectiveness of the system.

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Finally, not least of Field's accomplishments is establishing a more sophisticated basis from which to view black life. Among other things, she indicates that slavery as a precapitalist system engendered attitudes that were uncongenial to the market economy toward which Maryland was progressing. Failure to understand this fact means failure to appreciate properly the activities of the freedmen and to assess accurately contemporary reports of these activities. Freedmen did not behave like *homo economicus*—the money-motivated, disciplined proletariat required by capitalistic production—because they were not such. Their mentality was that of subsistence peasants who worked for another only to secure a basic minimum and who spent the balance of their time in independent pursuits essential to self-sufficiency. The freedmen were also not the only southerners or Marylanders to exemplify this attitude. The advance of capitalism meant complaints about, and erosion of, the life style of anyone (oystermen, for example) who did not fit the mold. This attitude, therefore, was a function not of race but of a historical condition.

Considering Maryland from about 1850 to the turn of the century, during the transition from slavery to freedom, from tobacco growing to grain cultivation to truck farming, and from precapitalist to capitalist modes of production, this book clearly has implications beyond its geographical locale. Students of slavery and of the South cannot afford to overlook it.

DANIEL C. LITTLEFIELD
Louisiana State University

CHRISTOPHER L. MILLER. *Prophetic Worlds: Indians and Whites on the Columbian Plateau*. New Brunswick, N.J.: Rutgers University Press. 1985. Pp. x, 174. $27.00.

Significant contributions to history do not always supply new data; they sometimes rearrange known information to make interpretive explanations more comprehensive. This small volume accomplishes the latter objective in a forthright and persuasive manner. Christopher L. Miller condenses familiar ethnographic material about twenty-three tribes, including Flatheads, Nez Perce, Spokans, and Cayuse. This capsule gives a solid base for understanding how Columbia Plateau Indians interacted with whites later on. Crucial to native stability even in precontact times was a task group system, an ad hoc coalition that emphasized intertribal cooperation. By the late eighteenth century this behavioral pattern was accompanied by a conceptual complexity that absorbed recent cultural stresses and anticipated further change. Such changes, foretold by local prophecy, seemed fulfilled by the arrival of American explorers (1805) and missionaries (1835). Christian emissaries harbored their own metaphysical perceptions that superficially resembled native expectations. For a time the views seemed compatible and mutually beneficial, but, when each side slowly recognized irreconcilable differences, natives and whites quickly polarized along cultural lines. By the late 1840s, native perceptions of whites through prophetic lenses had deteriorated. This led to a collapse of their web of tribal relations whose mutual cooperation had given them strength. Cohesion dissolved, lands were lost, reservations loomed, and new prophetic dreams issued in the Nez Perce War and further ideological mayhem.

The main point of this succinct presentation is that world views affect peoples' perceptions; our understanding of differing metaphysical realities sheds light on the dynamics of cultural interaction. This principle helps unravel ethnocentricity and xenophobia and says something new about the death of Marcus Whitman (1847) and eventual cession of right-of-way for a northern railroad (1855). There are a few weak spots, however, attention to which might have improved this impressive treatment of complex materials. The enunciated native prophecy shows unmistakable signs of white influence. Such a syncretistic formulation is notable for having already progressed along lines of intercultural exchange. What earlier conditions produced that adjustment, and how does its comparatively autonomous world view compare with revisions made after whites came in force to the Columbian Plateau? Further, the author's references to Christian millennialism reflect scant appreciation of differences between premillennial and postmillennial thinking or between Protestant and Catholic millenarian views. He also settles for blurred categories by equating an ordinary sense of cultural superiority with millennialism and by allowing congressmen and the secretary of war to speak as prophets. A final drawback is that, since the book stands on the observation that people view experiences through their own perceptions, it is gratuitous to criticize them for not seeing their counterparts more "objectively." Still, Plateau Indians were, for reasons derived from native priorities, pawns in a game of sectarian chess and federal encroachment. This extended essay restores important dimensions to the story, and it will be genuinely appreciated for doing so.

HENRY WARNER BOWDEN
Rutgers University

DAVID J. WEBER. *Richard H. Kern: Expeditionary Artist in the Far Southwest, 1848–1853*. Albuquerque: University of New Mexico Press, for the Amon Carter Museum, Fort Worth, Texas. 1985. Pp. xii, 355. $45.00.

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In 1975 the Amon Carter Museum of Western Art in Fort Worth, Texas, acquired an album labeled "Memorabilia," with twenty-one finished watercolors and four pencil sketches made by Richard Kern between 1849 and 1853. Kern and his brother Edward were important to the development of the American West because they served as artists, scientists, draughtsmen, and mapmakers for several early western expeditions sponsored by the Army Topographic Corps. Lithographs based on their work were published during or shortly after their lifetimes in such volumes as James H. Simpson's *Report from the Secretary of War . . . of the Route from Fort Smith, Arkansas, to Sante Fe, New Mexico* (1850). Henry Rowe Schoolcraft's six-volume pioneer study, *Indian Tribes of the United States* (1851–57), and the widely circulated *Pacific Railroad Reports* (1855). Some of the Kerns' work thus provided the first visual images of the land and peoples of certain areas of the West. Since then many scholars have reproduced some of these lithographs; William H. Goetzman, for example, includes the Kerns' work in *Army Exploration in the American West (1803–63)* (1959) and *Exploration and Empire: The Explorer and the Scientist in the Winning of the American West* (1966).

"Memorabilia" contains hitherto-unknown works, thus prompting David J. Weber to undertake a study of the artist. The album was probably put together in 1852–53 when Kern lived in Washington, D.C. By this time he had already become an expert on the West, having accompanied John C. Frémont's fourth expedition in 1848–49 and Simpson's to the Navajo country in 1849; spent more than a year in New Mexico during which he made a number of journeys into the surrounding area; and accompanied Lorenzo Sitgreaves to the Pacific. Sixteen of the watercolors are beautifully reproduced in color in Weber's book. All are attractive, but some are exceptional and evocative, such as the *Valley of Taos, Looking South*, and *La Parroquia, Santa Fe*. This last watercolor, along with the *Interior of the Jesuit College, Chihuahua* and *Interior of the Church of San Miguel, Pueblo of Zuni, N.M.* (really Iglesia de Guadalupe), are also valuable documents showing these churches essentially as they appeared in the mid-nineteenth century. Other works of Kern are also reproduced—there are 151 illustrations in addition to the color plates. Most are more factual than evocative. Weber carefully evaluates the scientific or historical import of each. Kern had a keen interest in ethnography, and his drawings of Indian habitations—including *Ruins of an Old Pueblo in the Cañon of Chelly*, the *Pueblo of Zuni*, the *Estufa or Council Chamber at Jemez*, and an untitled drawing of ruined pueblos of Wupatki—are important documents.

Weber places the new material in context, traces Kern's routes in detail, identifies the nature of the commissions, and relates this information to nineteenth-century publications as well as to more recent literature on the region. He disentangles the work of Richard Kern from that of his brother Edward. The Kerns, men with lively minds, were raised in Philadelphia when that city was a center for both science and art; both studied and then became teachers at the Franklin Institute. Richard Kern's expertise as artist-scientist and mapmaker developed during his participation in various expeditions. Weber's account of these conveys the intimate nature of the day-to-day trials and triumphs, the problems, the accomplishments, the interaction between the men, and the varying contacts with different Indian groups. The maps they prepared were the foundations on which the next generations would build. Neither Richard Kern nor his brother made any single major contribution to the studies of botany, zoology, anthropology, or archaeology of the Southwest, but the piecemeal accomplishments of men like the Kerns helped shape the corpus of material on which these disciplines have been built. Richard Kern's career was cut short when he was killed by Indians in Utah in October 1853, while on an expedition to survey the proposed railroad route to the Pacific along the thirty-eighth parallel. He was but thirty-two years old.

Weber's well-written study has comprehensive footnotes and an excellent short bibliographical essay. One cavil is that in several instances Weber notes that the place names given by Kern for his watercolors or drawings are not those now in use or are incorrect, yet the picture captions give only Kern's titles. The correct identification in the text is often separated from the picture: plate 14, for example, appears on page 64 and the correct identification on page 155. Without cross-referencing, confusion will arise.

JESSIE POESCH
Newcomb College
Tulane University

DAVID L. ROWE. *Thunder and Trumpets: Millerites and Dissenting Religion in Upstate New York, 1800–1850.* (American Academy of Religion Studies in Religion, number 38.) Chico, Calif.: Scholars. 1985. Pp. xiii. 188. Cloth $24.95, paper $16.95.

David L. Rowe's work is the first professional monograph on Millerite Adventism, a movement of thousands—some say hundreds of thousands—of Americans who predicted the end of the world in 1843. It is a good book: balanced and deeply researched, it transcends the Adventist apologetics and village-atheist snickering that have typified writing on the Millerites and places them firmly within the history of American Protestantism.

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Rowe demonstrates that the prophet William Miller was not a crank but the carrier of a long tradition of popular apocalyptic study. In 1818, after reading for two years, he cracked the biblical code: the end would come in 1843. There was nothing extraordinary in that; the Bible had led other searchers to the same date. But this time the discovery launched one of the most spectacular episodes in American religious history. Rowe explains the transition from solitary prophecy to mass movement under the terms "millennialism," "revivalism," and "pietism." Premillennialism gave the movement its intellectual framework. Revivalism provided both an environment of religious excitement and a method of spreading the word, and pietism accounts for the intensity of commitment, the diversity within the movement, and some of its antinomian excesses.

Within that explanatory framework, Rowe links Millerism to concurrent religious developments, and his most valuable and imaginative pages trace the movement's ties to a broad antiformalist revolt against denominationalism, postmillennial, man-centered religion, and organized missions among what were becoming the mainline northern Protestant churches. When the Millerites turned their conservative attention from the churches to the world, they found further signs of decay: party politics, materialism and greed, extremes of poverty and wealth, dietary fads, New York City, banks, brokerage houses, corporations, and the governments that permitted and encouraged them. Rowe makes little of this catalogue, but it is intriguing nonetheless: the range of religious and secular developments that others called "progress" convinced the Millerites that the world was irredeemably corrupt and that Apocalypse was around the corner.

This book presents a thorough history of Millerism and a judicious analysis of what it meant. Many readers, however, will find that analysis too narrowly organizational and religious. Rowe does conclude with a brief discussion of the movement in social terms, but the attempt is half-hearted and, given the riches that precede it, disappointing. Despite its brief and sometimes promising forays into other territory, the book remains an institutional study of Millerite Adventism. But it is without question the best of those.

PAUL JOHNSON
University of Utah

C. C. GOEN. *Broken Churches, Broken Nation: Denominational Schisms and the Coming of the American Civil War*. Macon, Ga.: Mercer University Press. 1985. Pp. x, 198. Cloth $17.95, paper $13.50.

This is a case study of the relationships of religious ideas and institutions to antebellum American social realities. Specifically, it documents the role of the churches in signaling and reinforcing a growing sectionalism that eventually led to the Civil War. It compellingly suggests that a significant factor in the separation of the North from the South was the division into sectional factions of three large denominations: Baptists, Methodists, and Presbyterians. C. C. Goen claims that the moral chasm between North and South was both exposed and deepened by the actions of these divided churches.

The hypothesis shaping this study is that, when the churches divided, an important bond of national unity was severed; that the resulting forebodings of religious and political leaders regarding the coming war were well-founded; and that the schism of the churches foreshadowed and contributed to the eventual national tragedy. This is not a new hypothesis, but Goen is the first to argue the case with hard evidence.

The key themes of the book are set forth in the first four chapters. Chapter 1, "The Evangelical Bond," documents the church's role in creating a strong national bond by sharing a common evangelical heritage and a religious nationalism across sectional lines. Chapter 2, "Churches of the People," argues that the large popular denominations, because of their size, institutional strength, and representation of the nation as a whole, anticipated and engendered the national division by their own schisms. In chapter 3, "The Secession Scenario," we get a blow-by-blow account of the intramural fighting that led to a sundering of ties in Baptist, Methodist, and Presbyterian churches. Chapter 4, "The Broken Union," is a normative summation of the main argument of the book, which is that the denominational schisms did in fact presage and provoke the Civil War. The author's careful scholarship and controlled point of view give his conclusion plausibility.

The last chapter, "A Failure of Leadership," is a scholarly lament over the lack of moral guidance by the churches as the nation grappled with the moral problem of slavery. According to Goen, the churches in both North and South ended up reflecting the social values of their respective cultures with virtually no prophetic moral influence. The view that nineteenth-century evangelicalism was inflicted with romantic individualism and lacked social realism is shared by many historians. The inability of evangelical Christianity to view the moral evil of slavery in institutional terms is an old thesis. But Goen's case is weakened because he does not seriously come to terms with scholarship that argues that, at least in the North, a more profound social witness did take seriously institutional and systemic dimensions of moral issues. Furthermore, I think

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his treatment of Timothy L. Smith's thesis, which claims that evangelicalism and social reform came to grips with slavery, is inadequate.

There is another shortcoming. The author draws mostly on well-known primary sources and secondary works but ignores a wealth of unmined primary sources. Also, he gives surprisingly little attention to the furious debates that took place in religious periodicals. Hence, this book is valuable more for its conceptual contribution than for fresh historical data.

Despite these shortcomings, the book is eminently worth reading. The crisp and clear writing style makes it a joy to read. The major themes are well argued and grounded in solid historical data, making this book a significant scholarly contribution.

DONALD G. JONES
Drew University

CRAIG M. SIMPSON. *A Good Southerner: The Life of Henry A. Wise of Virginia.* (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1985. Pp. xviii, 450. $27.95.

Few, if any, Virginians played as dominant a political role in the Old Dominion in the generation before the Civil War as Henry A. Wise. Although remembered best for his encounter, while governor of Virginia, with John Brown in 1859, Wise in fact had been at the cutting edge of party politics and debate in Virginia since he entered Congress as a freshman representative in 1833. During the decade he served in Congress, Wise broke with Jackson over the Bank issue, helped found the Whig party in Virginia, battled acrimoniously with John Quincy Adams over the gag rule, and bolted the Clay Whigs to support the Tyler administration. Rewarded by Tyler with the post of U.S. minister to Brazil, Wise returned home to tower over the Virginia constitutional convention of 1850–51, which fashioned an intricate compromise that balanced additional representation for the white majority counties in the west with a system of taxation designed to safeguard the slaveholding interests of the tidewater counties in the east. His successful gubernatorial campaign of 1855 was the critical blow in breaking the momentum of Know-Nothingism in Virginia, and Wise, running as a Democrat, received the largest popular vote of any Virginia politician in any nineteenth-century election.

Wise, the son of a prominent Federalist family from Virginia's Eastern Shore, was a popular but enigmatic and volatile politician. He was, in turn, a Jacksonian Democrat, Clay Whig, Tyler Whig, Buchanan Democrat, and Grant Republican. At great risk to his political career he attacked the Atlantic slave trade in the 1840s, while minister to Brazil, but publicly flirted with reopening it in the U.S. in the 1850s. He despised sectional extremists but did more than any other individual to precipitate disunion in Virginia. His eccentricities, sudden shifts, deceptions, and outright lies present a major interpretive problem to any biographer.

It is a measure of Simpson's success that he offers a sophisticated explanation of Wise's public career that is compellingly integrated with his private life. The interpretive key for Simpson is the ambivalence of Wise—his deeply rooted uncertainties regarding authority, be it of slaveholding, the Union, or any dominant figures that threatened his fragile sense of autonomy. Subtly used on a subject who was anything but subtle, this notion of ambivalence acts not as a crutch to avoid seeking significant conclusions but as an analytical tool to reach them. What makes this biography work so well as guide to the politics and ideology of late antebellum Virginia is that Simpson persuasively links Wise's self-doubts with those of Virginia's ruling class of slaveholders. The result is both a superb biography and a fascinating study of a regional culture in crisis, one that was ever mindful of its fall from revolutionary glory and dominance but too proud and fearful to reverse that decline by embracing the program of economic modernization championed by Wise in the 1850s. This culture, like Wise himself, finally found in war a powerful emotional release from its self-imposed dilemma.

WILLIAM L. BARNEY
University of North Carolina

ELISABETH MUHLENFELD. *Mary Boykin Chesnut: A Biography.* (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1981. Pp. xv, 271. $20.00.

Had it not been for the work she did during the last years of her life, Mary Boykin Chesnut (1823–86) would not be remembered. From 1881 through 1884, she polished, revised, and altered her private Civil War journals, which were first published in 1905 as *A Diary from Dixie.* Elisabeth Muhlenfeld presents a chronological account of Chesnut's life and a thorough analysis of her unpublished novels and her famous diary.

Mary Boykin Miller grew up on a plantation near Stateburg, South Carolina, was educated in an elite girls' school in Charleston, and at the age of seventeen was married to James Chesnut, Jr., of Mulberry Plantation near Camden. Since her husband was active in the political and military affairs of the Confederacy, Chesnut mingled intimately with the elite of that government. She knew and recorded their political and personal dreams, their battle wounds and romances, and their successes and

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failures. Nevertheless, despite living in comfort as a slaveowner, she hated slavery, once noting, "The Bible authorizes marriage & slavery—poor women!—poor slaves!" (p. 103).

At times, Muhlenfeld fails to make her subject come alive as a human being. Chesnut was a complex, moody woman whose motivations and attitudes need more explanation than this biography provides. This weakness may be attributed in part to limited sources. Chesnut's reasons for feeling mismatched with her husband, the burden of her childlessness, and the strong possibility that her dislike of slavery was prompted by sexual jealousy of black females are subjects touched on but lightly or not at all. Sometimes reveling in her feminine role, sometimes feeling imprisoned by it, Chesnut found much cause for complaint. The reader is left wondering if Chesnut would have preferred the freedom to die on the battlefield, to make difficult political decisions, to rebuild ruined plantations, and perhaps to seek sexual gratification in the slave quarters.

The biographer is at her best when dissecting Chesnut's writings. Muhlenfeld analyzes Chesnut's unpublished fiction and shows how her techniques for writing fiction influenced the diary. Twenty years after the war, Chesnut removed personal details from her journal and reshaped her dramatic and valuable memoir of the Civil War. For almost a century, it has been accepted as a leading primary source for the study of that era.

Despite the weaknesses of this work as biography, it nevertheless commands the serious attention of historians and literary scholars. The details of Chesnut's life and the sharp analysis of her writings illuminate the chaotic and revolutionary age that produced both Chesnut and her journals.

E. STANLY GODBOLD, JR.
Mississippi State University

JOSEPH T. GLATTHAAR. *The March to the Sea and Beyond: Sherman's Troops in the Savannah and Carolinas Campaigns*. (American Social Experience Series.) New York: New York University Press. 1985. Pp. xvi, 318.

This interesting volume in the American Social Experience Series is a study not of military history but of social history. Joseph T. Glatthaar examines General William T. Sherman's operations in Georgia and the Carolinas through the eyes of the common soldier, whose viewpoint on most issues was entirely different from that of his superior officers. The author explores the attitudes of the men toward their comrades in arms, blacks, southern whites, foraging, pillaging, and battle.

Until the publication of this book no scholar, with the exception of the late Bell Irvin Wiley, had attempted a comprehensive social history of the Civil War participants. Wiley's *Life of Billy Yank* (1943) has fewer than two dozen references to the soldiers who marched with Sherman, and Wiley definitely concentrated his research on the Army of the Potomac.

The men who followed General Sherman to Savannah and then into South and North Carolina were totally different, Glatthaar points out, from those who fought primarily in the eastern theater. Sherman's army was composed of veterans who had learned soldiering through several years of vigorous campaigning in the West. In the process they developed a remarkable sense of self-reliance and self-confidence that enabled them to handle almost any type of situation. These traits were gained, however, at the expense of rigid discipline and smart, polished appearance. At the start of the march to the sea Sherman probably had the most experienced large command in United States military history, many of the men having reenlisted during the war for a second three-year hitch. These veteran troops made the difficult moves through Georgia and the Carolinas possible.

Even the most publicized aspect of these campaigns, the application of total war to three states of the Confederacy, was in part a product of the experienced character of Sherman's army. The general designed this innovative strategy to make the South "feel the hard hand of war" (p. 135), and his men after long years of hardship and sacrifice in the field were quick to adopt it. They saw total war as a means to exact revenge for the sufferings they had endured. Also, they agreed with their commander that this method of warfare would shorten the conflict and thus help preserve the Union. Consequently, South Carolina, referred to by the soldiers in blue as the hellhole of secession, was to pay a heavy price for her role in bringing on the conflict.

The in-depth look at the common soldiers who marched with Sherman marks this volume's contribution to Civil War literature. Glatthaar is the first historian not only to record fully what these veterans did but also to note why they did it. Since the author's primary concerns are ideas and attitudes, he neither condones nor condemns the actions of the men. Glatthaar quotes often and effectively from primary sources. But his exhaustive research has led to an imbalance in the book, almost one-half of which is devoted to notes, bibliography, and appendixes.

JOHN G. BARRETT
Virginia Military Institute

GARY W. GALLAGHER. *Stephen Dodson Ramseur: Lee's Gallant General*. Chapel Hill: University of North Carolina Press. 1985. Pp. xiv, 232. $19.95.

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Almost before the smoke-filled battle reenactment at Bull Run in 1961, the commercial orgy of the Civil War centennial (1961–65) had peaked. Anything about the Civil War had become saleable. An auto manufacturer offered a special model in "Confederate Gray" sold only in Dixie states. A Manhattan entrepreneur offered a canine "turncoat" for poodles—one side blue, the other gray. In the first week of production, Parker Brothers sold twenty-five thousand copies of a board game called "1863" and reaped success with others such as "Gettysburg." Magazines strained to find Civil War topics. The *Saturday Evening Post* featured "The Lost Rag Doll of Appomattox," which tracked down the odyssey of a doll in the house where General Lee surrendered. Television heralded "The Gray Ghost," a cavalier fantasia about rebel Colonel John Mosby, while the New York *Herald Tribune* introduced a syndicated cartoon feature, "Johnny Reb and Billy Yank." Yet the greatest deluge of Civil War lore came in the torrent of books. A favorite technique was to comb state libraries for a postwar reminiscence or wartime diary. Padded by an imaginative title, maps, and photographs, many items better suited for a journal appendix saw daylight in book form.

One of the gold mines of centennial literature was the biography of some usually obscure general officer, especially on the Confederate side. This was fertile ground, for Dixie fielded some 425 generals, most of whom are not household words today. Even the most devoted professional Confederate would be hard pressed to identify such semiobscure officers as Victor Jean Baptiste Girardey, Elkanah Brackin Greer, and Albert Gallatin Blanchard. Eager doctoral candidates seized on such military figures as fodder for a doctoral dissertation and later published the accounts with semiromantic titles. Other professional and amateur historians did the same, somehow believing that all rebel generals deserved a biography.

Thus was established the pattern of the Civil War centennial biography, a predictable volume whether the subject fought in gray or blue. Yet even the collegiate version of *Webster's* defines a biography as a "written history of a person's life." First, one assumes this involves a discussion of the formative days of the subject's life long before becoming a Civil War officer. A biography relates to the subject's personality, ambition, relationships with family and friends, and other personal topics. Second, a true biography would involve more than a chronicle of events. A true life study of a Civil War officer therefore is more than a day-to-day recounting of his whereabouts, interspersed with letters home. Such a recounting does not constitute biography, even when the author does much research or uses the aggravating device of calling the subject by a first name or nickname.

I am sure that Gary W. Gallagher has written the definitive history of the career of General Stephen Dodson Ramseur. Ramseur was certainly less obscure than many other rebel generals. Commissioned major general at age twenty-seven, he was the youngest West Point graduate to achieve that rank in the army before his death at the battle of Cedar Creek in 1864. Gallagher's research has been exhaustive, and his text is embellished with intricate maps and many illustrations. After reading the Ramseur biography, this reviewer knows everything anyone would desire to learn about where the subject was on any given day during his career in Robert E. Lee's army. But the book is a chronicle and not a biography. The approach dates back to the centennial writings. One still knows little about Stephen Ramseur.

THOMAS L. CONNELLY
University of South Carolina

ROBERTA SUE ALEXANDER. *North Carolina Faces the Freedmen: Race Relations during Presidential Reconstruction, 1865–67*. Durham, N.C.: Duke University Press. 1985. Pp. xviii, 238. $29.75.

Roberta Sue Alexander offers an intensive analysis of the efforts of North Carolina blacks and whites to forge a new system of social relations in the immediate aftermath of emancipation. Sharp conflict was endemic in this process, she argues, because whites attempted to preserve their dominance over blacks and former slaves sought independence from white control. Blacks made important gains, enjoying more stable family life, establishing their own churches and community organizations, and boldly asserting their rights as free men and women. Nevertheless, Alexander demonstrates that whites used their political and economic power to limit the gains: apprenticing of black children to white planters was extensive, whites terrorized blacks with little fear of legal retribution, black agricultural laborers remained poor and economically dependent, and most black children did not enjoy access to schools.

A number of Alexander's findings are particularly interesting. Her treatment of the black family includes an analysis of county marriage registers that suggests that interplantation marriage among slaves was more common than many scholars have suspected. Her discussion of the state's nascent black political leadership shows that, even in a state with a sizeable free-born black population, the vast majority of political leaders were freedmen. Nevertheless, these men included a large number of artisans and ministers, and many owned modest amounts of property. The author's extensive work with state

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criminal court records shows that freedmen found it almost impossible to obtain redress against white criminals and that in many counties blacks were indicted and convicted at significantly higher rates than whites. A surprising number of counties, however, record roughly equal rates for both races.

Alexander neglects several issues. North Carolina included tobacco- as well as cotton-producing regions, but the author does not consider whether differences in the tobacco and cotton economies created disparate patterns of labor relations. Nor does she do justice to the variety of sharecropping agreements and the sharp and persistent conflict between planters and croppers over specific terms of their relationship. Although I find interesting her guarded suggestion that the crime rate was relatively low in the postwar years, I am skeptical. She does not present comparative statistics that demonstrate that indictments and convictions per capita in 1865–67 were lower than in the antebellum years for which she has data. Moreover, she does not consider that the number of prosecutions and convictions (as opposed to crimes) may have been low in the postwar years because of the disorganized condition of local government and federal officials' assertion of jurisdiction over blacks.

This is, nevertheless, a well-researched, lucidly written study that contributes to the ongoing reappraisal of the consequences of emancipation.

DONALD G. NIEMAN
Kansas State University

PAUL ANDREW HUTTON. *Phil Sheridan and His Army*. Lincoln: University of Nebraska Press. 1985. Pp. xvi, 479. Cloth $29.95, paper $14.95.

No scholarly work on relations between Indians and the military in the post–Civil War period can avoid dealing with General Philip H. Sheridan. Yet no modern historian, until now, has provided us with a comprehensive, authoritative account of Sheridan's western military career. Paul Andrew Hutton's career biography should satisfy that need for several generations to come.

Like many West Point graduates in the 1850s, Sheridan began his military career on the frontier and served as an infantryman in Texas, California, and the Washington and Oregon territories. During the Civil War he achieved military greatness as a dashing cavalry officer. Since this part of Sheridan's career has been widely chronicled, Hutton devotes only nine pages to it.

Sheridan emerged from the war eager to apply his awesome military skills against western Indians. His appointment to head the Department of the Missouri, and then the larger Division of the Missouri, gave him the opportunity. Because of the strategic location of this command, his friendship with army commander William T. Sherman, and the unfailing loyalty of President Ulysses S. Grant and Secretary of War William Belknap, Sheridan was able to wield more authority over an extended period of time than any other frontier commander. He pressed these advantages to the limit in his zeal to open the West to white exploitation and thereby fulfill what he saw as the nation's mission.

As commander over the Great Plains, Sheridan planned all the major campaigns against the Plains Indians. The strategy and tactics of the Washita campaign (1868–69) set the pattern for the wars under his direction: hit the enemy in the winter, destroy their homes and property, and kill civilians and warriors alike ("The only good Indian is a dead Indian"). This kind of warfare was modern, ruthless, brutalizing, and successful. By 1880 almost all the Indians were either "good" Indians or rapidly approaching that state.

Sheridan did more than subjugate Indians. He assumed jurisdiction (unconstitutionally) over Chicago after its great fire, and his army crushed its strikes. He enforced Radical Reconstruction, played a role in the 1876 election crisis, and escorted foreign and domestic dignitaries throughout the West. This book also discloses Sheridan's career-long collusion with businessmen, demonstrating the extensive commingling of military and business interests in the Gilded Age. While commanding general of the army (1883–88), however, he successfully protected Yellowstone Park from commercial exploitation, a feat that is described as "his finest hour and greatest achievement" (p. 360).

Thoroughly researched in both primary sources and secondary works, this study reveals much not only about General Sheridan but also about the frontier military establishment. The exposure is not likely to enhance the reputation of either. Hutton superbly characterizes Sheridan's key subordinates and analyzes his relations with them. He concludes that all too often the pragmatic but vindictive general allowed his animosities and prejudices to interfere with the proper conduct of military and Indian affairs.

This book is interesting, well written, and generously illustrated. And, except for several typographical errors (three in one chapter alone), the publisher has produced a handsome volume.

MARVIN E. KROEKER
East Central University

WAYNE R. AUSTERMAN. *Sharps Rifles and Spanish Mules: The San Antonio–El Paso Mail, 1851–1881*. College Station: Texas A&M University Press. 1985. Pp. x, 367. $29.50.

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Establishing mail service from San Antonio to El Paso required hundreds of mules, thousands of tons of hay, hundreds of thousands of dollars, dozens of lives, and thirty years' perseverance by at least nine different contractors. This tenuous 650–mile segment was but a tiny portion of a network of mountain and desert routes that linked the Pacific coast with the rest of the American Union in the prerailroad era. Just how fragile were these bonds of communication becomes clear in Wayne R. Austerman's history of this one Texas mail route, where the ambitions of a few men challenged a landscape that remains astonishing in scale.

Unlike an immigrant train, which had to make one desperate lunge through the arid West, mail service entailed the creation of permanent stations, each stocked with fresh animals, food, and water, across a hostile desert filled with angry native peoples. Nothing but permanent settlement and local agriculture would bring real security to this mule-drawn transport system, yet development itself depended on communications with distant thriving communities. Not surprisingly, then, we learn that the El Paso mail probably never paid its contractors a clear profit but consumed fortunes in treasure and human energy. Without generous postal contracts the task might not have been tried, and, if the contractors never got what the service was worth, the post office nevertheless poured hundreds of thousands of dollars into this frontier adventure. (Federal subsidies to the westward movement did not all go to the railroads!) Unlike most stories of "progress," this one could not possibly end happily. Technologically tied to a hostile land, the stage line was preyed on throughout its life by bandits, Indians, and the climate itself. Eventually, the steam locomotive, its supplies on its back, would speed past the wastelands to those scattered centers where men and women found more to life than mere subsistence.

Austerman has probably exhausted the sources in reconstructing this narrative. Anecdotes and personalities weave together in a pleasing style, while the reader rides the mail coaches back and forth across the desert and occasionally joins the contractors for junkets to Washington. Two serious flaws interfere with the success of the story. First, Austerman gives no credibility to the Indians whose domain the expressmen violated. Austerman's Indians move as part of the environment without intelligent material or cultural motives. "Stinging cold" seems to "invigorate" them one day; warm weather revives their "deviltry" the next (pp. 75, 79). Only dimly do we see their purposeful (and relatively successful) resistance to this final encroachment on their crumbling world. The second shortcoming mirrors the first: Austerman never convincingly shows us why his contractors repeatedly wagered their lives and their fortunes on a business that suffered continual losses. Without a sustained discussion of motives on each side of the conflict, the tales of ambush and murder on the mail route become flat and tiresome, and the perseverance of the contractors seems mindless.

Readers unacquainted with the subject may find this book as repetitious and monochromatic as a desert landscape. Others, already enamored of the subtle beauty in the arid regions and their history, will find it more rewarding. On a western scale this is local history, and its dedicated particularism contributes to our understanding of the larger scene.

JOHN LAURITZ LARSON
Purdue University

ANNE M. BUTLER. *Daughters of Joy, Sisters of Misery: Prostitutes in the American West, 1865–90*. Urbana: University of Illinois Press. 1985. Pp. xx, 179. $16.95.

Drawing largely on the court records and newspapers of twelve western states, Anne M. Butler shows that the daily lives of prostitutes were nasty, brutish, and short. She challenges those historians (Judith Walkowitz, Marion Goldman, and Ruth Rosen) who, she unfairly suggests, have depicted the prostitute as a "professional who chose her job after carefully crafting her career plans" (p. 154).

Dividing her 155 pages of text into six chapters, Butler demonstrates that prostitutes lived everywhere in the rural and urban West, came from a variety of ethnic groups rooted in preindustrial economies, and were poor and young. Although many of them were mothers, all their relationships "floundered in a torrent of destructive . . . experiences" (p. 46), a view that is in contrast to Goldman's wished-for picture of sororal ties in accordance with Carroll Smith-Rosenberg's ideal. Butler also shows that prostitutes were trapped by poverty and social ostracism and were subject to alcoholism, drug addiction, violence, and suicide. The degradation of these women was reinforced by politicians and law officers for whom the legal/illegal status of prostitutes was in some sense a resource. Finally, despite much irrelevant evidence, the author argues fairly persuasively that prostitution was a de facto part of military policy, whereby army authorities maintained prostitution while denying that they did so. Army posts employed prostitutes under several misleading classifications, including "laundresses." One suspects that the laundresses whom Linda Kerber describes following the revolutionary army probably were prostitutes, although it seems likely that they, like Butler's laundresses, did the laundry, too.

Butler makes several original points, showing the existence of small-farm brothels and comparing the

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reform convent in St. Paul, Minnesota, to the bordellos whence it drew its "Magdalens." Her account of the broken adult lives of prostitutes complements Ruth Rosen's account of the homes from which they came. Butler can write concisely and well, but, too often, her prose is overcome by indignation. She provides no sense of change over time, and she dispenses recommendations on how churches, newspapers, and communities should have behaved to ameliorate prostitution.

But what is most surprising is that Butler ignores Goldman's *Gold Diggers and Silver Miners* (1981), dismissing it in a footnote. (Nonetheless, Goldman has supplied Butler's book with an extremely generous blurb). In contrast to Butler's large canvas, Goldman wrote a local history of prostitution in Virginia City, Colorado, during the same period. Goldman's detail, her attention to questions of social stratification and general gender arrangements, and her methods of exploiting census materials and diaries could have been invaluable to Butler. Butler's account of the repeated suicide attempts of Lottie Pickett illustrates "the problems of the prostitute" (p. 29), but the existence of "Suicide Frenchy" in Goldman's Virginia City and a context of "almost unbelievable" suicide rates for prostitutes (*Gold Diggers*, p. 134) point to a much more arresting historical dimension to the issue, that of type.

Nonetheless, we must be grateful to Butler and these other historians for establishing the history of prostitution as a subject of significance to gender arrangements as a whole, and I look forward to the integration of these different historical aspects.

G. J. BARKER-BENFIELD
State University of New York, Albany

JACK S. BLOCKER, JR. *"Give to the Winds Thy Fears": The Women's Temperance Crusade, 1873–1874.* (Contributions in Women's Studies, number 55.) Westport, Conn.: Greenwood Press. Pp. xix, 280. $35.00.

In late 1873 a number of women in Hillsboro, Ohio, organized marches, sit-ins, pray-ins, and sing-ins to persuade local merchants to quit selling liquor. By mid-1874 the Women's Crusade, as it came to be called, had spread to more than nine hundred localities in thirty-one states. Although Ruth Bordin, Barbara Epstein, and Jed Dannenbaum have examined this movement recently, Jack S. Blocker, Jr., tells us in greater detail just who these women were and why they protested against liquor. We learn that the crusade began in Fredonia, New York, but that Hillsboro had better publicists; that it was spread by the national press, which often was hostile; that it was concentrated in Ohio; that crusaders were native-born, middle-class Protestants; that religious leaders divided on the issue of female activism; that the crusade drew its greatest support proportionately in small towns, where up to one-half of the women participated; that a high rate of participation was closely related to success in closing a town's liquor outlets; and that in places where male politicians supported prohibition crusaders shifted from protests to electoral campaigning.

Blocker insists on analyzing the crusade in the terms used by its participants: it was rooted in the suffering of women whose male relatives drank to excess. In an excellent case study of Washington Court House, Ohio, he shows that a surprisingly large number of crusaders had relatives with drinking problems. Furthermore, no woman could feel secure that her family would escape such a problem in the future. Indeed, the more property a family had, the more a woman could lose to the Demon Rum; it was women from the wealthiest 30 percent of the population who became crusaders. Blocker shows that the protesters were more concerned with drying out their hometowns than with wider protest; the Hillsboro women even allowed local liquor dealers to sell remaining stock to dealers in Cincinnati.

Some of the statistics pose problems. Blocker, for example, carefully counts the number of licensed liquor dealers. That statistic is important only if we assume that the number of dealers is related to consumption; at least one modern study has shown that there is no such relationship. To take another case, Blocker examines arrest records, although he is unable to ascertain the social, economic, or political milieu in which the arrests took place. One wonders whether these statistics are appropriate for the purposes for which they are used. Instead of such statistics, one wishes to know more about the depression of 1873 or about Ohio's prominence in the crusade. We should not, however, be too hard on a book that contains so much rich data and so many useful insights. The overall interpretation, carefully constructed from manuscripts as well as statistics, is cautiously sound and often illuminating. Blocker not only has shown how the crusade led to the creation of the Woman's Christian Temperance Union in 1874 but also has rescued this movement from historical neglect.

W. J. RORABAUGH
University of Washington

DAVID R. COLBURN. *Racial Change and Community Crisis: St. Augustine, Florida, 1877–1980.* (Contemporary American History Series.) New York: Columbia University Press. 1985. Pp. xi, 258. $30.00.

In 1963–64 a series of racial demonstrations and counterdemonstrations wracked the small, conserv-

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ative, tourist-oriented city of St. Augustine, Florida. Major civil rights leaders, including Martin Luther King, Jr., joined the demonstrators, and several outspoken white supremacists came to rally segregationists. In the aftermath of the resulting imbroglio, which occurred as Congress debated and passed the Civil Rights Act of 1964, blacks achieved a number of symbolic but not always substantive victories, and the city experienced a deep, enduring racial embitterment.

David R. Colburn relates the story of these events with authority and interest. His account is based on interviews with large numbers of participants, on records generated at all levels of government, including the Justice Department and the FBI, and on other pertinent sources. Approximately half the text describes the confrontations between blacks and segregationists, focusing on the demonstrations, the organizational efforts that produced them, and the activities of segregationists who opposed them. The other half is a more analytical treatment of certain aspects of the subject, chiefly the role of federal judge Bryan J. Simpson, the activities of white business and church leaders, and the immediate aftermath of the crisis.

Colburn's treatment is knowledgeable, and his judgments are balanced. He gives more than the usual attention to the leaders and purposes of the segregationists, and he questions some of the strategies and tactics of the blacks. None of his interpretations is entirely new, but he has documented much more thoroughly than anyone else the connection between King's efforts on behalf of the 1964 civil rights law and the conduct of the Southern Christian Leadership Conference at the St. Augustine demonstrations. He also knowingly details differences within the white and black communities of St. Augustine and explains rather convincingly why the confrontations produced such lasting bitterness.

Yet the book has some problems. If the subtitle does not violate truth-in-packaging legislation, this legislation should be amended to cover such misrepresentations. The study is of the years 1963–65, not 1877–1980, which may be one reason that Colburn is more satisfactory at narrating events than at explaining the crisis. It is still not clear to this reviewer just why St. Augustine endured so much agony when other cities with similar racial histories and circumstances had far less difficulty with racial change. Perhaps part of the explanation lies in aspects of the subject Colburn does not treat, especially the racial ideas and concerns of the majority of citizens, black and white, who remained outside the events he describes. Also, Colburn's treatment of limitations of white business and church leaders is quite interesting, but he has no parallel in-depth discussion of black moderates and conservatives, whose differences with militants undermined the militants' position during and after the crisis. Still, the book is well worth the read.

I. A. NEWBY
University of Hawaii

MICHAEL R. HEINTZE. *Private Black Colleges in Texas, 1865–1954.* (Texas A&M Southwestern Studies, number 3.) College Station: Texas A&M University Press. 1985. Pp. 211. $27.95.

Of the eleven private or church-related colleges for Afro-Americans in Texas, most were founded by religious denominations soon after the Civil War. Paul Quinn College was opened in Austin, Texas, in 1872 by black circuit riders of the African Methodist Episcopal Church, while a group of black Baptists, who were disgruntled about white control over educational opportunities for Afro-Americans, started Guadalupe College in Seguin in 1884. In 1905 the black Baptists opened Butler College in Tyler, Texas, where in 1894 the Colored Methodist Episcopal Church (the black offshoot of the Methodist Episcopal Church, South) had founded Texas College.

Several white Protestant denominations opened the remaining institutions. Wiley College in Marshall, Texas, was founded in 1873 by the Freedmen's Aid Society of the Methodist Episcopal Church (North), while the Congregationalist American Missionary Association opened Tillotson College, also in Austin, in 1881. The American Baptist Home Mission Society, which overall sponsored five black schools, chartered Bishop College in Marshall in 1885, and in 1886 the Presbyterian Board of Missions for Freedmen opened Mary Allen College (for black females) in Crockett, Texas. St. Philip's College was founded in San Antonio in 1898 by the Episcopal Church, and the Board of Missions of the Disciples of Christ established Jarvis Christian College in Hawkins in 1912. The Freedmen's Aid Society of the Methodist Episcopal Church sponsored a second school, Samuel Houston College, which finally settled in Austin in 1900 after moving from several other locations.

Despite the financial and spiritual support offered by many predominantly white groups and organizations, these black colleges overwhelmingly manifested Afro-American self-determinist and educational values. Because of the similarities in origins, objectives, and clientele, the colleges have much in common, enabling Michael R. Heintze to discuss them as a group and analyze their collective development. He examines the curricula, which tended to be "classical" with heavy doses of religion and theology. Industrial education was added in the early 1890s, but most of these institutions did not succumb to the lure of funds from industrial phi-

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lanthropists who wanted to redirect the programs along the lines of the Hampton and Tuskegee institutes. The faculty and administrations of schools opened by white denominations started out all white, but by the 1920s and 1930s even the boards of trustees began to respond favorably to the increasing demands for black self-determination. As qualified Afro-Americans were increasingly available to assume positions of authority, there were fewer apparent differences between the schools founded by black and white denominations. Heintze discusses several of the more outstanding faculty members and administrators who faithfully served these generally fragile institutions.

The precarious financial conditions for these schools were major challenges for their administrators. Initially, 70 to 90 percent of their budgets was from denominational and private sources; as these funds became increasingly inadequate, the administrations came to depend more on tuition and fees, grants, and organized philanthropy to avoid economic disaster. Over the period student life changed significantly, moving from strict religious authoritarianism to relatively unrestricted environments.

The sources for this study include administrative records, college catalogues, and interviews. Heintze makes some comparisons with other black schools but not with the white religious schools in Texas (or elsewhere), which were also rigidly authoritarian, financially insecure, and at times unaccredited. Heintze calls his study "revisionist," but it really reflects what black scholars have said about these institutions from the beginning of this century: "The private black colleges . . . made vital, unique, and lasting contributions to the advancement of blacks in Texas" and throughout the United States (p. 185).

V. P. FRANKLIN
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New York, New York

DAVID JOHN HOGAN. *Class and Reform: School and Society in Chicago, 1880–1930*. Philadelphia: University of Pennsylvania Press. 1985. Pp. xxv, 328. $30.00.

This is a case study of Progressive reform in schooling and, more generally, of the social matrix in which Progressive ideology was formed and of Progressivism's effect on social class structure in Chicago. David John Hogan employs a variant definition of "social class"; for his purposes, it is a dynamic and fluid concept that is neither categorical nor *a priori*. He describes Progressive ideology as having had three strands: justice, democracy, and efficiency. These differing priorities sometimes led to disagreements among Progressive reformers.

In 1880 the "great transformation" in production and markets created huge factories and market economy. The market economy led to the appearance of Populists, the Socialist Labor party, the Knights of Labor, and the Eight-Hour Movement, as well as to strikes, boycotts, and the Haymarket Square bombing. Defenders of radicalism pursued law and order, temperance, evangelistic religion, and charity reform; John Dewey perceived the change and proposed an educational remedy. Hogan argues that Progressivism in part resulted from social crisis and in part was a perpetuation of older American ideologies. Originating in the changes brought about by the market economy, it supported and furthered these changes.

Progressive "child savers" secured the prohibition of child labor, instituted compulsory education, established kindergartens, and supported child-centered pedagogy. Urban reforms to protect adolescents from vice, crime, and temptation impinged on the authority of the family. There is no substantial evidence that these reforms otherwise essentially altered the lives of Chicago's working class: the market revolution and the Industrial Revolution shaped working-class lives. Compulsory education laws did keep some children in school, but increasing numbers of youths between fourteen and seventeen years of age stayed in school beyond the minimum leaving age; declining demand for child labor, increasing use of accreditation, a growing perception of the value of schooling, and the increase of parochial schools in ethnic Catholic parishes accounted for that.

Despite opposition from Progressives with other priorities, vocational education became an important part of schooling in Chicago. Vocational education, which developed in response to demands for social efficiency, followed from the Industrial Revolution, the use of mechanical power, specialization, and the principles of scientific management. Changing conditions of labor made skills less important than attitudes. Practical, vocational education was triumphant in Chicago, although Progressives most concerned about democratic education opposed it. Vocational education, vocational guidance, and psychological testing were all triumphs for social utility and efficiency and furthered the structuring of class society.

This account of the centralization of control over Chicago schools is somewhat familiar. In spite of the opposition of Progressives who favored social democracy, who favored control of Chicago schools was centralized for efficiency. Callahan's "vulnerability" thesis did not apply: the Chicago superintendents were not vulnerable or fearful.

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To generalize too much from Chicago's history would be unwise, and Hogan usually avoids that. The organization of this study is imperfect, and the customary condolences for the shortcomings of editors are in order. But Hogan does illuminate the origin of social Progressivism in Chicago. Social class was redefined in the rationalization of education, but the "mirror image" of social control was avoided. Hogan has beneficially revised educational revisionism.

H. WARREN BUTTON
State University of New York, Buffalo

DONALD P. GAVIN. *John Carroll University: A Century of Service.* Kent, Ohio: Kent State University Press. 1985. Pp. xvi, 553. $27.50.

In 1986, John Carroll University (whose name was changed from St. Ignatius College in 1923) will celebrate its first one hundred years of service to Cleveland and northern Ohio. To prepare for that event, the university historian, Donald P. Gavin, has written an institutional history that exhibits some of the strongest and weakest features of this genre. Alumni and patrons will appreciate the author's attention to the minutiae of locale and personality. We learn, for example, that the original name of the school's athletic teams was the "Saints" but that it was changed in the mid-1920s to the "Blue Streak" and a decade later to the "Blue Streaks." During the same years the college orchestra was hailed as among the best undergraduate ensembles in the nation, but, as the "fortunes of the Cleveland Orchestra rose," those of the St. Ignatius "declined" (p. 181). And Father Frederick Odenbach became Cleveland's expert on exotic animals, fiery phenomena in the sky, and the difference between earthquakes and local blasting projects. These and other anecdotal inclusions should help to alleviate what friends of the university consistently lamented over the years: the failure of the larger community to note Carroll's presence and contributions.

To give broader significance to this particularistic approach, the author begins most chapters with sections that put the matter to be treated into a wider context of both Jesuit and Catholic, both local and national, educational efforts. Thus, chapter 14 opens by telling us that institutions of higher education in the 1960s "could not escape the influences of the Civil Rights movement and the Viet-Nam war" and that, for Catholic universities, "Vatican II raised questions about the role of a Catholic college or university in the modern world." Carroll's administrators therefore introduced such radical changes that its founders "might still have recognized many familiar things" in 1960, but ten years later "they would have been hard put" to do so, and "most probably they would not have approved the changes" (p. 358).

The final chapter provides a helpful summary of most of the major themes in Carroll's history. These include its transition from a West Side college founded by German Jesuits to a more cosmopolitan East Side university with a new name and a reputation for not cooperating with other Cleveland Catholic schools; the power of the local bishop to frustrate or facilitate such developments; Carroll's ability to place its graduates, who were drawn largely from lower-class ethnic neighborhoods, in the professions; and its struggle to maintain a specifically Jesuit and Catholic quality of academic and moral training. Drawing predominantly on administrative files, the author is as ready to suggest failure as he is to underline successes.

JOHN WHITNEY EVANS
College of St. Scholastica

DANIEL HOROWITZ. *The Morality of Spending: Attitudes toward the Consumer Society in America, 1875–1940.* (New Studies in American Intellectual and Cultural History.) Baltimore: Johns Hopkins University Press. 1985. Pp. xxxi, 254. $26.50.

Daniel Horowitz has made creative use of diverse sources in order to integrate several fascinating strands of American cultural history. In the process, he offers significant insights into contemporary values. This book is about consumption and how it has changed from the nineteenth to the twentieth century, but, more important, it is about moral attitudes toward consumption—how some people make judgments about how other people spend their money. Horowitz notes that, although consumer culture involves powerful people and institutions trying to impose their vision of a good society, consumers have always had their own kind of power: they use their expenditures to give a meaning to life that is derived from their own personal, social, and cultural histories. The tendency toward consumerism—the desire for comfort, status, and pleasure—existed long before the impact of industrialization, a mass market, and advertising.

Since the nineteenth century, American social critics have written about changing standards of living in moralistic terms, emphasizing the dangers of decadence, the loss of self-control, and the desirability of a nonmaterialistic life style. The conservative morality of the nineteenth century was concerned about the profligacy of the immigrant and working classes and saw the bourgeois way of life as superior. In the twentieth century, modern morality focused on the self-indulgence of the middle class and extolled the virtues of an "authentic" folk cul-

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ture. But both kinds of morality called for the pursuit of nonmaterialistic goals and saw the consequences of a higher standard of living as problematical.

In developing this theme, Horowitz juxtaposes social criticism from the antebellum period to the 1930s and actual standards of living as portrayed by household budget studies, which provide a record of the expenditure patterns of working-class families. The critics range from Henry David Thoreau and Alexis de Tocqueville to Robert Lynd and Helen Lynd and include many lesser-known names. The primary criteria for inclusion is a preoccupation with those issues that Horowitz addresses. Horowitz uses several landmark studies of family expenditures, beginning with Carroll D. Wright's 1875 household budgets study, done while he was head of the Massachusetts Bureau of the Statistics of Labor, and including others from the Progressive era, the post–World War I years, and the 1930s. His analysis shows how Americans spent their money, but it also shows the emergence of a changing definition of economic need, as reflected in spending patterns and in the changing attitudes of the professionals and social critics.

Horowitz's book is valuable not only as an intellectual study of social criticism and changing cultural values but also as good family history. The economic role of women and children, the importance of "respectable shelter," the changing nature of miscellaneous expenditures, and the tensions between private and public solutions to the economic and cultural problems of family life are some of the themes that Horowitz addresses. Although some may find his family focus too narrow—he admittedly limits himself to urban, nuclear families with a male head of household employed full-time—his findings have broad implications for further work on a historical issue of contemporary significance.

WINIFRED D. WANDERSEE
Hartwick College

NAOMI R. LAMOREAUX. *The Great Merger Movement in American Business, 1895–1904*. New York: Cambridge University Press. 1985. Pp. xii, 208. $29.95.

This book, although not without flaws, represents a pathbreaking effort and may well become the model for the "new" business history. Naomi R. Lamoreaux treats an important subject in a fashion that is both analytical and historical. The findings are important, and the work could not have been done had the author not been able to blend an economist's formal models with a historian's ability to discover and interpret primary source material.

Lamoreaux's subject is the wave of mergers that swept the American economy at the beginning of the present century. Unmatched before or since, the so-called first merger wave produced 157 consolidations in a single decade; almost 90 percent of that total was crowded into the five years from 1898 to 1902. Of those consolidations that the author has been able to examine in detail, almost three-quarters controlled two-fifths or more of their industries, total output and more than two in five controlled over 70 percent. The industrial structure of the American economy was never again the same.

Economic historians have long been aware of the importance of those mergers, but, with the exception of Ralph Nelson's careful quantitative study, *Merger Movements in American History, 1895–1956* (1959), little work has been done at even the most aggregative level, and almost no one has offered any explanation of the successes and failures of the new industrial giants. This latter question serves as the central focus of Lamoreaux's study. Her work begins where Nelson's left off, and she goes beneath the general trends to examine the histories of two particular industries, steel and papermaking. She draws heavily on the records of individual firms as well as on the trade press and other more standard sources. Although the models she employs are drawn from simple partial equilibrium analysis, her results are neither partial nor simple.

Lamoreaux concludes that successful mergers (where success is defined from the point of view of the firms, not of the consumers) occurred in industries that were marked by significant levels of excess capacity, the result of "too rapid" growth, and a technical or market structure that permitted the new consolidation to assume the role of the "dominant firm" in the industry. If her conclusion is supported by an analysis of the experience of other industries, we will have a much better understanding of the path of American industrial development. As far as steel and paper are concerned, she has made her case.

The work does, however, have some problems. In particular, although the explicit use of economic analysis is crucial to the argument, the economic reasoning is sometimes tortuous and at other times thoroughly confusing. In her discussion of cartel behavior, for example, Lamoreaux never specifies the model that leads her to conclude that price stability is a characteristic of a successful cartel. More bothersome is what appears to be a continued confusion between costs and expenses and between profitability and liquidity. This confusion leads, at one time, to a cryptic analysis of the relationship between a manager's failure explicitly to recognize depreciation charges and the fixed-cost hypothesis, and, at another, to a strange discussion of the course of operating profits in a period of rapid technical

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change (most economists would argue that a firm will continue to earn normal profits on its operations as long as its capital is valued downward to reflect the reduced earning power of the old plant and equipment).

Although the problems raised by the author's use of theory are more than annoying, they do not detract in a major way from the book's importance. Lamoreaux has at the very minimum provided a partial explanation of the structure of the nation's economy. Hers is a book that every student of late nineteenth- and early twentieth-century American development must read.

LANCE DAVIS
California Institute of Technology

NANNIE M. TILLEY. *The R. J. Reynolds Tobacco Company.* Chapel Hill: University of North Carolina Press. 1985. Pp. xxi, 706. $35.00.

Nannie M. Tilley, whose *The Bright Tobacco Industry, 1860–1929* (1948) established her as the foremost historian of that product and industry, completed *The R. J. Reynolds Tobacco Company* in 1964, whereupon a new company administration, apparently displeased with the finished product, consigned it to the company archives. To say this is the best study we have of an individual tobacco company is deceiving, because it is the only one ever undertaken. The suppression of Tilley's book, however, had to do not with the work's quality but with the corporate problems of the period and the usual paranoia business historians confront in dealing with corporate executives, who start out thinking it would be fine to have a company history but develop second thoughts along the way.

Tilley has written a large, old-fashioned history, which is to say that it will appeal more to scholars than to general readers. There are sections on labor relations, advertising, manufacturing, organizational changes and the rationales for them, leaf processing, and contests between Reynolds, American Tobacco, and Liggett and Myers. Readers eager to follow a single topic—be it corporate development, the triumph of cigarettes over plug tobacco and cigars, or the social significance of smokes—will find interesting and perceptive chapters dealing with each. The health issue receives scant attention because the story is taken only to the early 1960s, when that matter was just surfacing.

The Reynolds family settled in the Blue Ridge Mountain area of Virginia, near the delightfully named Poor and Nobusiness Mountains. The Reynolds were involved in tobacco almost from the beginning, but the origins of the company can be traced to 1875, when Richard Joshua Reynolds invested $7,500 in an enterprise for the manufacture of plug, by far the dominant tobacco product at that time. R. J. dabbled in other investments, including railroads and land, and became a political force in the Winston-Salem area of North Carolina, one of the better tobacco-growing regions of the nation. There were literally hundreds of producers, many of which became part of the giant American Tobacco Company being formed by Reynolds's nemesis, James Buchanan Duke. Reynolds's firm became part of Continental Tobacco, an American Tobacco Company subsidiary organized to take control of plug, and at one time had over five hundred brands on the market.

Reynolds chafed at this control and welcomed freedom after the American Tobacco Company was broken up in 1911. Four independent companies—American, Reynolds, Liggett and Myers, and P. Lorillard—emerged under terms of the dissolution, with Reynolds as the only one without a cigarette. This was rectified in 1913 with the introduction of Camel, the first mass-marketed brand. This marketing technique altered the face of the industry and hastened the switch from plug tobacco and cigars to cigarettes. In this section, which is the best in the book, Tilley documents the role of advertising and World War I in the growing popularity of cigarettes and describes the competition between Camel, Lucky Strike (American), and Chesterfield (Liggett and Myers). After chronicling the cigarette wars of the 1920s and 1930s, the work tends to trail off and, as indicated earlier, stops at 1960.

This work confirms Tilley's reputation as the premier historian of the tobacco industry. Despite its belated publication, her book is certain to become a standard reference for our generation.

ROBERT SOBEL
Hofstra University

PHILIP G. TERRIE. *Forever Wild: Environmental Aesthetics and the Adirondack Forest Preserve.* (American Civilization.) Philadelphia: Temple University Press. 1985. Pp. ix, 209. $24.95.

As the largest, arguably the wildest, and one of the most controversial natural areas east of Minnesota, New York's Adirondack country has not suffered from lack of scholarly attention. *The Adirondack Park* (1978) by Frank Graham, Jr., gives detailed coverage of the political history surrounding the 1885 establishment of the Adirondack Forest Preserve and the subsequent embodiment of the "forever wild" concept into the New York State Constitution in 1894. Alfred Lee Donaldson wrote a comprehensive two-volume *History of the Adirondacks* in 1921, and William Chapman White contributed *Adirondack Country* in 1954. In addition, there are several shorter publications and two long dissertations on

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the subject. What Philip G. Terrie proposes to add is a history of what he calls "environmental attitude." Terrie, formerly a curator of the impressive collections of the Adirondack Museum, sees himself as a historian of changing ideas. He knows that, although the land itself changes slowly, ideas about its significance have changed radically and rapidly.

Terrie's thorough research shows that hostility and fear of wilderness marked the early history of human contact with the Adirondacks. Gradually, appreciation of the region's wildness entered the descriptions, but even the appreciators, Terrie argues, retained a substantial amount of the old uneasiness. Love of wilderness was difficult to square with the nineteenth century's enthusiasm for progress and development. Even the preservation movements of the 1880s and 1890s ignored the wilderness and focused on the protection of watersheds and the prevention of unwise lumbering practices. Not until the 1930s did a clear desire for a permanent wilderness resource in northern New York appear well defined in public attitude. The rise of ecology and environmentalism in the 1960s added still other chapters to the Adirondack story. Commendably, Terrie brings his account to the present, and he recognizes that ideas about both wilderness and the Adirondacks are by no means fixed and final.

Definitions dominate the start of this book. Terrie understands that wilderness is a subjective concept, a state of mind, rather than a condition of geography. "Wilderness," he writes, "is in the eye of the beholder. The landscape constitutes a wilderness to the extent that people perceive it to do so" (pp. 11–12). Consequently, it is surprising to find Terrie, just a few pages earlier, opining that anyone who appreciates "lofty summits" but not swamps or unscenic spots "does not really love and respect wilderness" (pp. 8–9). Although this point of view may characterize Terrie's own attitude, using it to define wilderness violates his principle of subjectivity. If someone happens to think that mountains, but not mundane places like swamps, are wilderness, then this is their definition. Rather than lecture them on right-thinking, Terrie, as a historian of ideas, might be better advised to respect the subjectivity he describes.

This book introduces the reader to figures closely associated with the Adirondacks, both famous people such as Ralph Waldo Emerson and lesser ones such as Ebenezer Emmons, Verplank Colvin, and William H. H. "Adirondack" Murray. The strength of Terrie's book is his textual analysis of these and other observers' accounts. No previous history goes into the literature with as much detail and perception.

Terrie is less successful in keeping to his stated intention of not repeating the well-documented political history of Adirondack protection. And his book draws perhaps too heavily for its intellectual framework on the work of Hans Huth, Leo Marx, Peter Carroll, Barbara Novak, and the present reviewer. But in another sense what Terrie has done is to apply the broad cultural generalizations others have developed to a regional literature. The successful continuation of environmental history depends on this kind of refined focusing.

RODERICK NASH
University of California, Santa Barbara

THOMAS D. CLARK. *The Greening of the South: The Recovery of Land and Forest.* (New Perspectives on the South.) Lexington: University Press of Kentucky. 1984. Pp. xvi, 168. $20.00.

Thomas D. Clark wrote this book out of "personal conviction that the South's land and renewable forest resources are its most durable birthrights" (p. xiv). An active timber producer himself, Clark has crafted a forceful statement that should leave no reader doubtful about the regenerative role of woodlands in regional history.

The towering and botanically diverse forests encountered by the earliest settlers remained largely intact through antebellum times, despite persistent encroachments by pioneer farmers and loggers. The momentum of forest utilization, however, accelerated around 1880 with the influx of "carpetbaggers of the woods" who acquired vast acreages and began systematically reducing trees to boards, ship timbers, and sawdust piles. This colorful logging and sawmill era, here broadly portrayed without local detail, made fortunes for entrepreneurs and provided cash income for laborers accustomed to less. By the 1920s much of the lowland pine belt was skinned and laid open to erosion and wildfire; a similar juggernaut cut a swath through Appalachian hardwood forests. Many transient lumbermen moved on to the West, leaving behind a scene of desolation.

Clark clearly laments the attendant environmental and social disruption, but he does not permit moral indignation to overshadow his analysis of inexorable change. He finds seeds of recovery, naturally enough, in the forest itself. The process was nudged along by forest scientists, mainly outsiders, who chipped away at regional ignorance of silviculture, by armies of the Civilian Conservation Corps working on newly created national forests, by unprecedented public efforts to rehabilitate the exhausted Tennessee Valley, by native lumbermen like Henry Hardtner who advanced forest conservation through private demonstration, and by chemist Charles Herty of Savannah, whose research

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and publicity helped usher in the regionally transforming pulp and paper industry.

In concluding chapters titled "The Grand March South" and "Rearranging the Land," Clark shows that scientific forestry became a pivotal element in emerging postwar prosperity. The influx of corporate talent and capital (Champion, International Paper, Weyerhaeuser, Mead, and St. Regis are prominent examples) roughly coincided with a maturing "second" forest, an instructive gift of nature. Thanks also to favorable geography and cheap labor, what resulted was a proliferation of efficient wood-using industries to compete with traditional lumber mills for sustainable sources of wood fiber. Sprouting pine seedlings, perennial challengers on King Cotton's old fields, were nurtured through fast rotations, the harvested logs transported by pulpwood trucks to costly state-of-the-art mills. Marginal croplands once abandoned for taxes are now growing trees again, and the timber income ripples out through southern society.

The players in this drama are mostly forest workers, scientists, bureaucrats, and businessmen. Clark's book is not a morality piece with corporate heavies and environmentalist heroes. The tears occasionally shed for ravaged monuments and lost folkways are wiped away with thoughts of full grocery carts, bank deposits, and restored land values.

Some will challenge Clark's cheerful assessment of forestry's progress. What, they will ask, about the dispossessed and the lost "safety valve" role of southern woodlands that accompanied changing ownership? What are the ecological dangers of monocultural plantation forestry? Some readers will note typographical errors, light punctuation, and superfluous index entries attributable to the publisher. What survives the criticism, however, is a seasoned, regional interpretation of forest history, a much-needed synthesis related in splendidly robust prose.

RONALD J. FAHL
Santa Cruz, California

CHARLES E. TWINING. *Phil Weyerhaeuser: Lumberman.* Seattle: University of Washington Press. 1985. Pp. xvii, 401. $25.00.

Charles E. Twining's biography of John Philip ("Phil") Weyerhaeuser, Jr., a third-generation member of the well-known lumber family, adds to the growing list of personal and company histories of the forest products industry. Born to wealth and influence and educated in private academies and institutions of higher learning, Phil and his brother, Frederick K. Weyerhaeuser, carried on the family tradition of gradually adding to the firm's timberlands and its manufacturing facilities. Modest and self-effacing, Phil served as the key administrator for the firm from 1933 until his death in 1956. The first "full-fledged production chief executive" for the timber company (p. 95), Phil guided its fortunes through the depression, the Second World War, and the great boom in lumber production after 1945.

Written in an easy, conversational style, this volume will be useful to business historians and to those enamored with the life stories of America's captains of industry. For this is a sympathetic biography of an ever-patient executive who, according to the author, constantly battled the "labor problem," fended off government efforts at regulation, and dueled with "socialist-minded" Forest Service officers like Ferdinand Silcox. When unions gained recognition in the Weyerhaeuser mills, it was "costly to workers" (p. 153); when presidential elections in the 1930s and 1940s did not follow the corporate script, we are told that "Phil and his colleagues had grown accustomed to such disappointments. Life went on" (p. 302). Compelling though such anecdotes may be, one hardly anguishes with the executives in the Weyerhaeuser family who are constantly on the move—to Sun Valley, southern California, the Mediterranean, and other spas for the wealthy.

Although this biography is too long, its major problem is one of scope. It is written from the perspective of the Weyerhaeuser archives and, even more narrowly, from the papers of the principal protagonist. As with other accounts of this type, readers are led to believe that the Weyerhaeuser presence was always a benign and benevolent one. Twining makes no indication, except by inference, of the firm's powerful influence, along with other lumber industry leaders, in shaping the Lumber Code Authority under the National Recovery Administration, in forging the policies and practices of the War Production Board during the Second World War, or in redirecting the discussion of timber regulation from the federal to the state level where the large companies could more easily affect the outcome.

Twining, like Weyerhaeuser's public relations apparatus before him, praises the reforestation work at the company's Clemons Tree Farm. Left unsaid, however, is any reference to the several studies of the Elma area (the location of the tree farm), investigations that reveal the wrenching social and economic costs left in the wake of cut-and-run practices.

In short, we know a great deal more about the personality and business management style of this prominent member of the Weyerhaeuser family. But we already have before us a wide range of similar works on the forest products industry—biographies, company and trade association histories, and other studies, some of the more important ones done under the auspices of the Forest History Society. It is time that scholars moved beyond the

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commissioned civic celebrations apparent in so many of those publications to more important questions. We need to get away from the conventions of the older corporate histories and biographies and begin to examine how these emerging multinationals fit within the evolution of modern capitalism and how this has affected timber-dependent populations. In that way we can learn more about the broader significance of these corporate groups.

WILLIAM G. ROBBINS
Oregon State University

JUDITH E. SMITH. *Family Connections: A History of Italian and Jewish Immigrant Lives in Providence, Rhode Island, 1900–1940.* (State University of New York Series in American Social History.) Albany: State University of New York Press. 1985. Pp. xii, 228. Cloth $39.50, paper $12.95.

Contemporary historical scholarship on American immigrants has documented the centrality of the family in immigrant life. Relatives were at the heart of a complex migration network that assisted newcomers in finding jobs, homes, and a margin of economic security. Judith E. Smith's book on Italians and Jews in Providence, Rhode Island, both reinforces the importance of a family-based culture in immigrant settlement and pushes scholarly understanding to a higher level. By effectively combining oral history interviews with surviving Italians and Jews, quantitative data from the Rhode Island state census returns, and a stimulating array of photographs, she has produced a first-rate case study of immigrant family life in an urban environment.

This study makes a number of good points about family relationships. The book demonstrates that a collective or cooperative approach to survival characterized Italians and Jews prior to departure from the homeland. This is a point scholars have forgotten. Smith is astute, moreover, in refusing to accept the notion that immigrant culture was static. She cleverly details the manner in which families remained central to, but not unchanged by, the experience of the first generation. Thus, long-standing patterns of mutual assistance that had characterized the relationship between brothers and sisters in the homeland were replaced in America by expectations of aid between parents and married children. Smith's argument that the context and meaning of family life continually evolved in America is a good one.

Although some of the patterns of immigrant family life described by Smith are well known to specialists in the field, her book breaks new ground as well. This study of Providence goes further than other studies of its kind in suggesting how and when the vigorous family economy of the first generation began to wither. Smith, for instance, illustrates how the changing economic structure of a city led to a widening gap between the occupational experiences of the first and second generations of Italians and Jews (despite relative continuity in class standing). By 1940 it also became clear that parents could expect less support from their progeny; and mothers with young children began working outside the home in greater numbers. Finally, family-owned business establishments were progressively undercut by the growth of modern forms of retailing.

The cumulative result of this penetrating probe into the lives of Providence's immigrants is a richly detailed portrait. Ordinary immigrants are shown to be responding in complex ways to changing economic and social structures. One can admire not only the resourcefulness of these Italians and Jews but also the scholarship of Judith Smith.

JOHN BODNAR
Indiana University

DONALD T. CRITCHLOW. *The Brookings Institution, 1916–1952: Expertise and the Public Interest in a Democratic Society.* DeKalb: Northern Illinois University Press. 1985. Pp. xiv, 247. $23.00.

The story of the Brookings Institution is not, perhaps, among the more important possible subjects in American history. Donald T. Critchlow's treatment, however, brings the history of Brookings alive and makes it, just as he hopes, one with broader implications for the study of American politics and power.

The heart of the book is a straightforward study of the institution. It includes thoughtful, detailed, and well-researched reports of Brookings's work in the New Deal and on Indian policy, giving the reader a good sense of Brookings's experts at work on concrete policies. The book is at its best in its discussion of the formal and informal processes of internal governance and external relations with government, especially from the 1920s through the 1950s. Its lengthy discussion of the two central figures in the history of the Brookings Institution—Robert S. Brookings, the businessman who was the essential founder, and Harold G. Moulton, the economist who long ran the institution—is fascinating and very nicely done.

Critchlow argues that Brookings the businessman was committed in practice to Progressive values of an independent, administrative polity, whereas Moulton proved to be much more a politician, pursuing his own political values while naively (and conveniently) seeing them as one with Progressive norms of value neutrality and social science expertise. This should be ironical only to those who do not

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appreciate what Critchlow's account of Brookings teaches in a larger sense. It shows, Critchlow argues, that corporatist or structuralist theories of the Left, which suggest that institutions of the Brookings type will automatically be servants of a liberal capitalist order, are doubtful. Matters are much more complex than that—or were at least in the case of Brookings. The Brookings Institution was not controlled by big business or big businessmen. Much more crucial to Brookings's story were the elitist Progressive ideology of wise, independent experts (with all the illusions it can foster) and the particular character and beliefs of individual decision makers.

Perhaps the book could have done without the author's penchant for normative declarations on behalf of his version of radicalism, an equalitarian populism unsympathetic to any elites, whether Moulton and other social science "experts" or "the Rockefeller crowd" (p. 33). But at least one knows what Critchlow's biases are. Altogether, this is an impressive, well-written piece of research, about much more than this skeptic originally expected.

ROBERT BOOTH FOWLER
University of Wisconsin

GRANT WACKER. *Augustus H. Strong and the Dilemma of Historical Consciousness*. Macon, Ga.: Mercer University Press. 1985. Pp. xv, 206. $17.95.

This well-written little book is an attempt to place a prominent American Baptist theologian into intellectual perspective by examining his views on a problem that concerned many serious evangelical intellectuals in the late 1800s: the threat of idealism, subjectivism, and historicism. This is no small task in the space of less than two hundred pages, and one of the most significant shortcomings of this book is that it contains too little of everything. The discussion of historicism in religious scholarship is brief and derivative, as is the discussion of the response of most evangelical Protestants to the new criticism. Grant Wacker does little more than summarize the work of George Marsden, Ernest Sandeen, Sydney Ahlstrom, and other scholars. Today it is well known that evangelicals varied widely in their approaches to historicism. The most serious and searching of them attempted some accommodation; the most reactionary treated the new historical biblical scholarship as the work of the devil. But in between were a host of possibilities. Augustus Hopkins Strong, who was serious and searching, came much closer to the first of these alternatives than to the second, although late in life he showed increasing sympathy for the growing Fundamentalist movement, to which historicism in biblical scholarship was generally anathema.

But perhaps this book's greatest weakness is the brief and sketchy treatment of its principal subject. Strong is an enigma in the American Protestant tradition—supremely well but conservatively educated; a great administrator of a prominent evangelical institution but unafraid of taking risks, even if they compromised his orthodox credentials. As president of Rochester Seminary, he hired Walter Rauschenbusch and kept him on, the heresy of the Social Gospel notwithstanding. The result of this brave act was that Rauschenbusch was able to spend his career as a professing Baptist, thereby giving much more credibility to the Social Gospel than if it had been the product of yet another critic from outside the church.

Wacker discusses Strong's evangelical response to the intellectual world of the late nineteenth century with only the barest mention of the most significant fact: Strong was an evolutionist. One cannot understand the evangelical answer to historicism without considering the evangelical response to the theory of evolution by natural selection. Both heresies threatened evangelical thought in the same way—by substituting relativity and change for what orthodox Christians had generally perceived to be as absolute and timeless. More important, historicism and evolution were closely dependent on one another, particularly in religious scholarship. To be an evolutionist, one had to reject the view that every book of the Bible is inerrant, and historical criticism facilitated this. One of Strong's greatest contributions to American evangelical thought, overlooked by Wacker, was his attempt to be simultaneously an evolutionist and a systematic, evangelical Protestant theologian. As a result, Strong faced one problem that other evangelical evolutionists, such as Asa Gray and James McCosh, never did. McCosh and particularly Gray, who was a botanist, could confine their attempts to harmonize evolution and Christian orthodoxy to a few "big" questions about the sovereignty of God and the development of humankind. Strong, in contrast, had to make the theory of evolution by natural selection consistent with an entire theological system. To be sure, this effort was sometimes strained beyond credulity. In the 1890s, for example, he began to argue that the theory of evolution and genetic science explained, far better than the Bible, how the fall from grace affected all of humanity: Original Sin was an acquired characteristic. Preposterous as that argument may sound, it reveals something essential about the character of the man who made it and about his brave attempt to face both historicism and Darwinism, which for him were two faces of the same challenge.

HERBERT HOVENKAMP
Hastings College of Law
University of California

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ROBERT H. FERRELL. *Woodrow Wilson and World War I, 1917–1921.* (New American Nation Series.) New York: Harper and Row or Fitzhenry and Whiteside, Toronto. 1985. Pp. xii, 346. $19.95.

The first volume published in the New American Nation Series was Arthur Link's *Woodrow Wilson and the Progessive Era* (1954). The editors of the series then promised that that volume would be followed by one on the First World War and the collapse of Wilsonian idealism. Robert H. Ferrell has now fulfilled that promise by skillfully integrating the various facets of wartime mobilization at home with events "over there" and does it with an authoritative yet sprightly style. His is one of the few attempts to pull together in a single volume all the strands that made up that mobilization. Frederick J. Paxson attempted it in a much-underrated study in 1939, and David Kennedy added the penetrating *Over Here: The First World War and American Society* in 1980.

Ferrell's volume, which is based on extensive research, begins with a brief treatment of how the United States became involved in the war and ends with the election of 1920. Ferrell makes many notable contributions, including his treatments of the aged prewar officer corps, the convoy system, the rejuvenation of the supply system in France, the dramatic arrival of the U.S. Second Division at Belleau Wood, the evolution of Wilson's concept of the League of Nations, and the various explanations for his alleged uncompromising personality. Throughout, the key figure is Woodrow Wilson whom Ferrell sees as a "master of American politics in his first term" but much less the master in the second: leading American people into the war, struggling rather haphazardly with mobilization, announcing war aims without consulting the Allies, committing "tactical errors" in preparation for the peace conference, stubbornly confronting the Senate over the treaty, and refusing to resign in the tragic last months in office. Yet, Ferrell concludes, "if the Wilson Administration had done nothing else in 1917–18 other than create the AEF, its reputation in history would be secure" (p. 15).

Ferrell does not hesitate to make controversial statements. He writes, for example, that "it is now clear" that Wilson "was unneutral from the beginning" and implies that from the beginning the president wished, even planned, to bring the United States into the war. The question is that, even if Wilson were unneutral, did he intend "from the beginning" to bring the United States into the war? Evidence that Ferrell himself provides seems to suggest a negative answer. Later he says that the shipping program "had no effect upon the war, except to consume great quantitites of scarce supplies and labor." Ships were coming off the ways in large numbers when the war ended, and, Ferrell adds, much construction "mindlessly" continued after the armistice (pp. 99, 102). But could the Wilson administration reasonably have been expected to plan on the war ending in 1918 and to know that there would be no need for ships to replace those being sunk? Moreover, many Americans including Wilson and his advisers were dreaming of a postwar merchant marine that could challenge Great Britain at least in Latin America. At other points Ferrell seems to be critical of Wilson for not sufficiently recognizing how American industry could contribute to winning the war, a view that seems to overlook the fact that much of the congressional criticisms of the mobilization was that the industrialists had too much to do with it.

Indeed, in view of the scope of the study and his skill in dealing with controversial questions, Ferrell's almost complete lack of attention to Congress is difficult to understand: much of the mobilization was based on congressional enactment, after all, and such enactments came out quite differently because of the sometimes-extensive congressional hearings and frequently emotional debate, little of which is cited, that the small-town but hardworking professional congressmen carried on.

It is always possible to be picky about a short book, even a superb one covering a large subject. Suffice it to conclude that Ferrell's ranks well with its twentieth-century predecessors in the New American Nation Series.

RICHARD L. WATSON, JR.
Duke University

DONALD J. LISIO. *Hoover, Blacks, and Lily-Whites: A Study of Southern Strategies.* (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1985. Pp. xxii, 373. $29.95.

In his latest work on Herbert Hoover, Donald J. Lisio examines Hoover's encounters with blacks, southern Republicanism, and racism. He asserts that the popular interpretation of Hoover as a lily-white racist who tried to force blacks out of positions of leadership in the southern Republican party is "inaccurate and woefully incomplete" (p. xiv). Rather, Lisio sees Hoover as a man of good intentions and high principles who wished to develop clean and efficient government in the South and to remove race as a basis for politics. Unfortunately, Hoover's efforts failed, and in the process he alienated blacks and many of the lily-whites.

The book begins with an excellent discussion of Hoover's work in supervising Mississippi flood relief in 1927. Subsequent chapters examine Hoover's southern strategies during his quest for the Republican presidential nomination, the election cam-

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paign, and his presidency. At times, Lisio's account of the labyrinthine details of southern Republican state politics will sorely test the patience of all but the most devoted specialists, but the persistent reader is rewarded by, for example, enlightening analyses of the prosecutions of Perry Howard, the black Mississippian charged with political corruption, and the fight over the nomination of Judge John J. Parker for the Supreme Court.

Lisio's research is impressive. He has made extensive use of materials in the Hoover Presidential Library and of several other manuscript collections. His notes fill seventy-two pages of fine print, and his eight-page bibliographical essay is a valuable guide to research on southern politics and racism during the Hoover years. There may be more to say on the details of Hoover's southern ventures, but it seems unlikely that many scholars in the near future will wish to go beyond what Lisio has done.

Despite his revisionist claims, however, it is not obvious that Lisio has given us a drastically altered portrait of Hoover. To be sure, considerable evidence indicates that Hoover's intentions regarding blacks were benign; Hoover was not a simple bigot. To that extent he should be viewed more favorably than he has been by his most extreme critics. But, as Lisio himself admits, much of the evidence is damaging. Hoover underrated and repeatedly snubbed the NAACP; he misunderstood and cut himself off from black politicians; he delayed making appointments favored by Robert R. Moton and other conservative black leaders, thus ultimately alienating even this sector of the black elite. He not only surrounded himself with assistants who were unsympathetic to black concerns but also was guilty in instance after instance of political ineptness that worked against his own supposedly good intentions. Most of all, he failed to understand the meaning and extent of racism in American life. Consider, for example, Hoover's "southern revolution." His idea for Republican reform in the South would, in theory, have accepted black voting and not sought to interfere with blacks' constitutional rights. But his plan called for keeping blacks out of key party positions and refusing to appeal to blacks as a unified racial group. Moreover, his policy required that no public explanations be made to blacks about the long-range benign intentions; they would just have to accept everything on blind faith. Hoover and Lisio distinguish this sort of reform from the tactics of the traditional lily-whites. But they are not convincing. For blacks it was a distinction without a difference.

Lisio ends with the observation that Hoover was more a victim of racism than an enemy of blacks and that his "venture into southern politics proved a sad encounter both for him and for black Americans" (p. 282). I agree. But, if the evidence and conclusions set forth in this book by a sympathetic revisionist are the most that can be said for Hoover's racial policies, then he surely needs no new enemies.

RICHARD B. SHERMAN
College of William and Mary

ROBERT J. NORRELL. *Reaping the Whirlwind: The Civil Rights Movement in Tuskegee.* New York: Alfred A. Knopf. 1985. Pp. x, 254. $19.95.

When white Redeemers in Macon County, Alabama, joined Booker T. Washington in establishing Tuskegee Institute in 1881, they sought to build an ideal model for preserving interracial harmony. Conservative whites promoted educational opportunities and, in return, expected blacks to abandon the civil and political rights they had exercised so fully during Reconstruction. Rather than relying on their former slaves' self-restraint, Alabama lawmakers had, by the turn of the century, amended the state constitution to disfranchise black citizens through the literacy test, poll tax, and grandfather clause. Virtually excluded from the ballot box and rigidly segregated, Tuskegee blacks heeded Washington's advice to lift themselves up economically and await the attainment of political and civil equality.

Robert J. Norrell evaluates how well they fared in achieving their goals. Tuskegee Institute and the Veterans Administration hospital created in 1923 provided an oasis of black economic independence in a desert of white supremacy. These black-controlled institutions not only fostered racial pride but also encouraged upward mobility. Having fulfilled Washington's prescription for success, blacks assumed whites would then open up the political process to them. Black Tuskegeeans discovered instead that, no matter how well educated or prosperous they became, the ruling elite did not believe that blacks were as qualified as the most illiterate and impoverished white to cast a ballot.

Hard as they tried, however, white leaders could not stop this determined black bourgeoisie from claiming the rights they had been promised. Tuskegee blacks pursued Washington goals in Du Boisian fashion. Guided by Charles Gomillion and the Tuskegee Civic Association, they relentlessly challenged disfranchisement in the courts and Congress from 1941 to the mid-1960s. Armed with favorable judicial precedents, backed by congressional legislation, and exerting collective economic pressure, Macon County blacks managed to register to vote and made the white electorate a minority at the polls. Gomillion's version of civic democracy preached interracial power sharing and allowed pragmatic white politicians to continue in office. Yet for many whites the prospect of black rulers was repulsive, and they either rejected biracial coalitions

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or moved out of the county. In the late 1960s a younger generation of blacks came to power, also repudiated Gomillion's ideal, and created a nearly all-black government.

Norrell offers a sensitively written account of the civil rights movement in Tuskegee. Clearly sympathetic to the black struggle, he nonetheless conveys the pain and anguish of whites whose racial paternalism blinded them to the changing realities in their communities. The author understands the complexities of race relations in Alabama's black belt and carefully analyzes the various shades of opinions held by blacks and whites. He cautions that the ballot has not been a panacea for black ills and suggests that issues of class have supplanted those of racial caste. This book reminds us that we cannot fully understand the development of the civil rights movement apart from the local efforts that originated and sustained it. Norrell demonstrates that, unless Washington's Tuskegee could be liberated, few other southern towns stood much of a chance.

STEVEN F. LAWSON
University of South Florida

JOHN H. EHRENREICH. *The Altruistic Imagination: A History of Social Work and Social Policy in the United States*. Ithaca: Cornell University Press. 1985. Pp. 271. $24.50.

It is often the case that sound and lively histories emerge from the experience of teaching courses for which there are no appropriate synthetic texts. This study of social work practice and public welfare policy in twentieth-century America reflects John H. Ehrenreich's efforts to inform the historical understanding of apprentice social workers at Smith College. Although a history with a conscious purpose and a self-conscious point of view, it happily avoids most of the liabilities that intensely presentist books often exhibit.

The story is propelled by a dialectical model of analysis that explores the persisting dual commitment of social workers to the amelioration and enhancement of the lives of clients, on the one hand, and to the achievement of reforms in the social environment, on the other. As the author declares at the outset of his story, "Again and again the tension between social policy and social work, between social reform and individual change, between explaining human misery in terms of the social environment and explaining it in terms of individual character flaws, has reappeared" (p. 10).

Within that framework certain underlying themes recur. One theme highlights Progressivism, the New Deal, and the 1960s, eras during which popular agitation provided opportunities for achieving reforms: "Without social movements there is no social reform" (p. 224). Such reforms "ostensibly aimed at the amelioration of social need" (p. 14) but often served the needs of the marketplace and advanced the self-interests of propertied and powerful classes. Social workers, in other words, provided essential human services but also acted as agents of social control. The search for professional expertise and status contributed to high levels of service but, at the same time, distracted social workers from asserting their broader public responsibilities. A final lesson, implicit throughout, demonstrates the failure of the profession, and of society generally, to engineer fundamental structural changes designed to redistribute social power: "That individual, personal 'powerlessness' is profoundly linked to objective, social powerlessness, and, conversely, that personal 'empowerment' is limited, if not impossible, in the absence of objective empowerment, is perhaps one of the greatest potential legacies of the sixties to social work practice" (p. 225).

Ehrenreich's analysis is informed by theoretical positions elaborated in the writings of Frances Fox Piven and Richard A. Cloward; footnote citations also indicate how widely he has read and synthesized secondary accounts in labor, women's, and welfare history and what sound use he has made of primary printed sources. Unhappily, he has not sampled the rich and extensive manuscript records now available in many research centers, not even the papers of those social work leaders he most admires such as Bertha Reynolds and Mary Van Kleeck.

Classic monographs in twentieth-century welfare history tend to emphasize either social work or social policy. Ehrenreich, to his credit, provides a critical, radical, and coherent synthesis of both strands.

CLARKE A. CHAMBERS
University of Minnesota

DEBORAH WELCH LARSON. *Origins of Containment: A Psychological Explanation*. Princeton: Princeton University Press. 1985. Pp. xvi, 380. $35.00.

One of the more encouraging historiographical developments of recent years has been the dialogue that is beginning to take place between American diplomatic historians and their colleagues in the fields of political science, international relations, and social psychology. Alexander George has done more than anyone else to encourage this trend; hence, it is fitting that Deborah Welch Larson's first book, which relates all three disciplines, is based on a dissertation written under his direction. Larson's book provides impressive evidence that these interdisciplinary contacts are at last bearing fruit.

Most of the disagreements historians have had over the origins of containment, Larson points out,

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have grown out of conflicting assessments of policy makers' motivations. But the resulting debates have often been sterile and unimaginative: historians have either taken the statements of policy makers at face value or imposed on them externally derived and sometimes crudely deterministic interpretive frameworks. There has been little recognition of the possibility that the *way* in which policy makers absorbed and analyzed information may have influenced the conclusions they reached.

This is where cognitive social psychology comes in. Its operative assumption is that *perceptions* of reality do not always accord with reality itself and that in fact misperception tends to follow certain experimentally verifiable patterns. Larson's assertion, in turn, is that familiarity with these patterns can provide historians with new and revealing ways of understanding problems like the origins of containment.

Experimental evidence suggests, for example, that information acquired through direct personal contacts is more likely to affect future attitudes than information absorbed indirectly, even though the latter may have a much sounder basis in fact. Larson uses this explanation to account for President Truman's persistent and otherwise puzzling conviction, after meeting Stalin at Potsdam, that he understood and could trust the Soviet dictator: the Russian autocrat reminded him of another familiar and fondly remembered authoritarian figure, Kansas City political boss Tom Pendergast. Stalin ultimately disappointed Truman, not by carving out a Soviet sphere of influence in Eastern Europe but by refusing to hold elections to make what he had done "respectable"—elections any competent "boss" should have been able to manipulate to produce desired results.

Larson also demonstrates, on the basis of psychological evidence, that it is possible for intelligent people to entertain contradictory positions at the same time, with the external environment determining which they choose to emphasize at any given moment. This phenomenon, she suggests, helps to explain Ambassador W. Averell Harriman's simultaneous beliefs in 1945 that the Russians were not to be trusted but that firm American action could still produce an improvement in relations. It can also account for the notorious tendency of both Roosevelt and Truman to reflect the views of the last adviser to talk with them, regardless of whether this position was inconsistent with those they had taken earlier.

Psychologists also suggest that individuals tend to "categorize" complex information. The result can be oversimplification—the application of shallow analogies like the one frequently made between Hitler and Stalin; it can also lead policy makers to "remember" events that never occurred. The best example here is Truman's retrospective but mythical claim to have issued an ultimatum to Stalin to get Russian forces out of northern Iran in 1946.

It is very much to Larson's credit that she does not try to claim too much for such psychological interpretations: she is careful to point out that they can only complement—not wholly replace—other forms of explanation, whether at the individual, institutional, ideological, or international systemic level. Nor does she claim consistency within the field of psychology for the various theoretical approaches she discusses: one of her most refreshing conclusions is the frank admission that cognitive dissonance theory, with its emphasis on an overwhelming drive to maintain intellectual consistency, is of little value when one is dealing with pragmatic politicians. Historians will be gratified to note as well that she does not neglect the archives: indeed, much of the persuasiveness of her argument comes from the care with which she has grounded it in a thorough familiarity with documents at the Truman Library, the National Archives, and other depositories too infrequently visited by other political scientists.

There are problems with the book. It focuses almost entirely on Harriman, Truman, Byrnes, and Acheson, thus neglecting others like Forrestal, Kennan, and Leahy, for whom such a psychological approach might also have been revealing. Larson tends to overlook the domestic political vulnerabilities Truman felt and the way in which these could have pushed him toward a policy of toughness toward the Russians. She draws on virtually none of the secondary work that has been done since 1980, which probably says something about the length of time it takes to get books into print these days. And there is no speculation about what insights this psychological approach might give us in understanding Soviet behavior, which was surely as subject to "information-processing" problems as was the behavior of the United States.

Still, this is a thoughtful, well-researched, and clearly written book. It reflects the growing maturity and interdisciplinary base of cold war studies. And its findings bear no little relevance to the behavior of more recent administrations, the present one not excepted.

JOHN LEWIS GADDIS
Ohio University

DON E. CARLETON. *Red Scare! Right-wing Hysteria, Fifties Fanaticism, and Their Legacy in Texas.* Austin: Texas Monthly. 1985. Pp. xii, 390. $18.95.

Students of the second Red Scare have often expressed the need for specific case studies. This book, the product of some thirteen years of research, is a careful examination of Houston during the Mc-

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Carthy era. Judicious, well written, and reliable, *Red Scare!* ranks among the top dozen books in the field and should serve as a model for similar efforts. Don E. Carleton worked extensively in a wide variety of archives, conducted nearly one hundred interviews, and mastered the best secondary materials. Houston is placed within a large historical framework but is also seen to contain distinctive features that made its Red Scare particularly fearsome.

After World War II, Carleton argues, Houston suffered from social strain, confusion, and fear stemming from rapid economic, demographic, and cultural changes. The city's power elite was insecure about its new wealth and status and deliberately gave credibility to a small fringe group of extremists, like the Minute Women of the U.S.A., who terrorized anyone thought to be on the Left. The cold war and the volatile rhetoric of Senator Joe McCarthy contributed to this tension. The handful of Communists and radicals that had excited the right-wing press and several patriotic organizations in the late 1940s had largely disappeared by 1950, when the Red Scare began. But, despite this, ultraconservatives battered New Dealers, Fair Dealers, civil libertarians, progressive educators, and others for several years with fanatical and irresponsible charges of Communist and pro-Communist leanings. Several prominent Houstonians on both sides of the struggle are described in depth, including Arthur J. Mandell, Jesse Jones, Oveta Culp Hobby, and Hugh Roy Cullen. An entire chapter is devoted to the outrageous and symbolic dismissal of George W. Ebey, deputy superintendent of schools.

The author stresses the role of the press in stirring up the Red Scare. He notes the upper-middle-class credentials of the right-wing extremists and points to racism as an integral part of the witch hunt in Houston. Moreover, he attempts, with considerable success, to distinguish McCarthyites from the Radical Right and the New Right.

I wish that Carleton had been more careful with the word "conservative," and he might well have made more use of the FBI materials currently available. But these are quibbles. He has written a splendid book that deserves the attention of everyone interested in the South and civil liberties.

THOMAS C. REEVES
University of Wisconsin, Parkside

RICHARD E. WELCH, JR. *Response to Revolution: The United States and the Cuban Revolution, 1959–1961.* Chapel Hill: University of North Carolina Press. 1985. Pp. ix, 243. Cloth $24.00, paper $9.95.

Richard E. Welch, Jr., is best known among diplomatic historians for his account of the imperialist debate at the turn of the century. In this book he shifts to a modern, still-controversial issue in the history of United States relations with Latin America, the reaction to Castro's revolution. What he has accomplished for the American side of the picture ably complements what several recent assessments of the Cuban revolution have done: analyze Castro's triumph and the confrontation with the United States in the perspective of the cold war.

Others have recounted the story of this antagonism, most of them contemporaries with a particular point of view to express or some vendetta to wage. In this study Welch has undertaken a different task—to explain why the United States as a society turned so vehemently against the Cuban revolution. In his charting of the Cuban-American confrontation, he follows a narrow course, trying hard to avoid the powerful ideological currents on both sides yet reminding the reader that one has to remember the cold war temper of those years to understand the Cuban-American conflict. Castro-mythologists will be disappointed in Welch's argument that Castro intended from the beginning to change Cuba internally (in brief, the U.S. never had much opportunity to direct Castro's revolution), and the most strident cold warriors will doubtless take offense at Welch's caustic remarks about the defects of both Eisenhower's and Kennedy's Cuban policy. Neither the reborn Eisenhower worshippers nor the surviving Kennedy idolaters will find much solace here.

The more substantial contribution of this book lies in its dissection of public attitudes about Cuba and, more important, how they affected the making of Cuban policy in these critical years. The earliest critics came from the Right, whose spokesmen warned against Castro's shift leftward early in the revolution and never slackened. On the other side were the tough liberals, who argued for Castro's revolutionary reformism (in Cuba and elsewhere in Latin America) but could never respond adequately to the Right's charge that Castro was intentionally taking Cuba into the Soviet orbit and, in so doing, was advancing Soviet power (which we were confronting globally) into "our" hemisphere. Thus, the liberals had to shift to a much tougher stance on Castro and prostitute their traditional convictions about intervention. The most notable liberal to convert was Arthur Schlesinger jr., who, in the White Paper on Cuba, advocated American deliverance of the Cubans from Castro's tyranny. The academic and "coffeehouse" community, enthusiastically supportive of the revolution, had no real impact on official policy. The press, like the general public, at first supported Castro and then turned against him.

This study, well reasoned and written in Welch's crisp prose, reinforces an observation made a cen-

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tury and a half ago by a French diplomat. To understand U.S. foreign policy, he observed, you have to understand its domestic politics.

LESTER D. LANGLEY
University of Georgia

ALONZO L. HAMBY. *Liberalism and Its Challengers: FDR to Reagan.* New York: Oxford University Press. 1985. Pp. viii, 386. Cloth $24.95, paper $10.95.

This readable political biography is in outline and emphasis analogous to Richard Hofstadter's *The American Political Tradition* (1948). That was a conscious decision of Alonzo L. Hamby, who writes to explain rather than judge as he traces the collapse of "collectivist democratic liberalism."

The subtitle may be disregarded, for only the period from 1933 to 1973 is covered biographically. Ford gets one paragraph, Carter ten, and Reagan four. Hamby devotes forty pages to FDR, fifty-two to Truman, thirty-one to Eisenhower, thirty-eight to JFK, fifty-one to Johnson, and fifty-seven to Nixon. The sketch on Martin Luther King, Jr., runs forty-four pages. Robert A. Taft, Joseph R. McCarthy, and Robert Kennedy are also discussed. Twenty-five rather speculative pages on LBJ's prepresidential origins follow Doris Kearns and Robert Caro admiringly. Readers of Allen J. Matusow's *The Unraveling of America* (1984) will learn little from Hamby on liberalism's decline.

Personalities, partisan politics, and presidential activity are the subject throughout. Intellectual and literary "challengers" to liberalism are missing. Thus, Herbert Hoover's postpresidential crusade as outlined in Gary Dean Best's two volumes, the widespread periodical literature of conservatism and libertarianism, and the effective program of the Heritage Foundation go unmentioned.

The FDR portrayed here is cut down to size. Roosevelt failed to achieve many of his most important immediate objectives and was "notoriously eclectic and nonsystematic" when approaching problems (p. 13). He and his colleagues challenged the depression with "a curious blend of halfway measures, irrelevant reforms, and inconsistent attitudes" (p. 22). Government was put into agriculture on a vast, permanent basis. The National Recovery Administration stifled competition, fixed prices, and made the problem of liquidated spending power worse. Full economic recovery was never achieved. Monetary tinkering failed to have a positive effect. His antibusiness attitude after 1935 inhibited economic expansion. In foreign affairs he followed the course of least resistance. Policy toward the Axis powers "involved back-pedaling, obfuscation, impossible promises, and, occasionally, outright deception"; he "cautiously steered the country toward war," more or less intentionally (pp. 40, 43). The unconditional surrender error and "Total War, Total Victory, Total Peace" policies required huge sacrifices and unrealistic reliance on unlimited idealism. FDR misunderstood the future international power structure, was ill equipped to deal with Stalin, relied on "atmospherics," and lacked the means to implement promised ends—"the restructuring of American life and the creation of a new world order" (p. 49). "Lyndon Johnson's agenda for America was far grander than FDR's" (p. 5). Although Hamby gracefully acknowledges Roosevelt's achievements, the author's indictment is surprisingly compatible with that of Edgar Eugene Robinson in *The Roosevelt Leadership, 1933 to 1945* (1954), a seldom-cited evaluation that forthrightly called that presidency a "tragedy."

President Truman does well. He was "one of the most effective of American presidents," who took "Roosevelt's ideals more seriously than had Roosevelt" (pp. 94, 93). Eisenhower, however, has little appeal for Hamby. Here a corrective might be Stephen E. Ambrose's *Eisenhower: The Presidency* (1985). Two recent books in the American Presidency Series, Donald McCoy's on Truman (1984) and mine on Johnson (1983), give somewhat different perspectives on these two executives; see also Henry P. Graff on LBJ in *The American Presidency* (1984). Hamby's treatment of the Kennedy brothers is mature. The Nixon sketch overstresses his anticommunist tactics and Watergate but still avoids dripping with hate. King fares well; Taft, poorly.

Some questions arise. When will the profession's crusade against Herbert Hoover (check Hamby's text and index for many examples) grind to a halt? Would other presidents have impounded the Nisei or used the atomic bomb against Japan? Did Eastern Europe and China absolutely have to be sacrificed to communism? Hoover, Eisenhower, and Ford kept us at peace, while Wilson, FDR, Truman, and Kennedy/Johnson did not—is this worth reflection? How much of liberalism's agenda was an appropriate solution for urban and minority problems in this half-century? And why is it that, even after the abundant failures of liberal presidents are catalogued, we still are told by the profession that they were "great" or "near great"? Here, I am just pondering a bit after enjoying a provocative book.

VAUGHN DAVIS BORNET
Southern Oregon State College

MANUEL H. PEÑA. *The Texas-Mexican Conjunto: History of a Working-Class Music.* (Mexican-American Monograph, number 9.) Austin: University of Texas Press. 1985. Pp. xi, 218. Cloth $19.95, paper $8.95.

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The serious study of Texas-Mexican music began in 1958 with Américo Paredes's *With His Pistol in His Hand*. More than history and folklore and neither positivistic nor impressionistic, his work is the rarest kind of scholarship in its mythic dimension. A milestone was reached in the 1980s with Chris Strachwitz's twenty-four-volume LP series, *Texas-Mexican Border Music*, that deservedly has influenced academic research. Another signal achievement is this monograph, which both breaks new ground and synthesizes scholarly trends of the past thirty years.

Manuel Peña's study examines the origins, rise, and decline of conjunto music as a working-class phenomenon. During the late nineteenth and early twentieth centuries, Texas Mexicans received northern Mexican musical culture in the form of European dances, string ensembles, and duet singing, with the guitar and accordion as prominent instruments. By the 1920s there was emerging a "Texas-Mexican folk, proletarian musical esthetic" (p. 47) that would characterize the classical conjunto featuring bajo sexto (twelve-string bass guitar), chromatic accordion, solo and duet singing, and the polka and cancion ranchera as primary musical genres. The tradition culminated in the 1960s with more elaborate instrumentation, trio harmonies, and eclectic repertory. In the 1970s, conjunto declined because of the dispersion of tejano workers, the arrival of undocumented aliens, the dissemination of Anglo popular music, and the failure of working-class culture to become part of the Chicano political movement.

Part 1 of this book contains some of the finest essays ever written on Greater Mexican popular culture; especially noteworthy is the idea of musical style as a system. But the social analysis in part 2 diminishes the book's impact. Peña invokes theories and models to justify only a few ideas, most of which are expressed in earlier chapters. For example, although he contends that conjunto musicians function as intellectuals, the reader has the impression that, if a theory of "organic intellectuals" were not available, Peña might not have made the point. Had sociology been woven as discretely as musicology into the main chapters or had the opening chapter comprised a jargon-free discussion of tejano society, the book would have been strengthened.

Part 2 also raises the question of why, of all the constituents in the social base, the author chose to elevate the working class to significance. Here, although I consider much of the theoretical context unnecessary, I would argue that the methodology falls short in not providing quantification and comparison of all social factors before a relationship among them is established or implied. Yet emphasis on the working class notwithstanding, a workers' aesthetic is suggested but not fully explored. More surprising is the lack of treatment of the subject's maximum power, the soul of the music—the suspended irony of the accordion run, the strained, echoed passion of conjunto singing. After all, it is the music, not the class structure, that the working people enjoy. Yet the book's uneven quality does not detract from its overall value. This is an important study of regional culture and should inspire similar efforts elsewhere.

HENRY C. SCHMIDT
Texas A&M University

CANADA

ANN GORMAN CONDON. *The Envy of the American States: The Loyalist Dream for New Brunswick*. Fredericton, N.B.: New Ireland Press. 1984. Pp. xii, 236. $12.95.

New Brunswick loyalists, who numbered fourteen thousand five hundred, were a diverse lot: ninety percent of them were American born, half were soldiers and half refugees, and the vast majority were plain folk. A small but influential elite, which came primarily from New England, did, however, exist, and Ann Gorman Condon focuses on these young, well-connected, well-educated men from prominent colonial families. To these men, who were just launching their careers and families, the American Revolution represented a dramatic disruption in their lives, and Condon skillfully portrays their reactions to it: their lust for revenge because of their treatment by the patriots, their frustration with the British conduct of the war, their feelings that their talents were being wasted, and their agonizing over whether to leave America or to stay.

Historians of the revolution will be especially interested in the author's conclusion that the loyalist leaders shared a conservative ideology and a common vision of an ideal society. They envisaged a hierarchical society—headed by an aristocracy of former military officers, landowners, and office-holders—an established church, and a government in which the appointed branches were strong and the judiciary independent. Basic to this dream was an integrated and nurturing empire that promoted colonial order and development. Yet loyalists shared the patriots' confidence in the colonists' "capacity to direct their own political affairs" (p. 65), and they insisted that the power of the governor and his local advisers be increased and that the colonies control taxation, except for trade regulation.

When New Brunswick became a separate colony in 1784, the elite had a chance to put its ideals into practice. They strove to create a deferential, ordered, and culturally sophisticated New Brunswick that would be "the envy of the American states." Its

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success would justify the choices made by the loyalists during the revolution, provide leadership opportunities for their sons, and attract the better sort of Americans disillusioned by the anarchy that loyalists were convinced would engulf America. Stumbling blocks to realizing this dream were economic hardship, growing opposition from other loyalists, and the neglect of the British government.

Condon's book is a good account of the elite; it is also well written and based on extensive research, although recent literature in the bibliography is not always integrated into the text. The plain folk, however, get short shrift. The preface mentions "the tension between the aristocratic dreams of the Loyalist leaders and the democratic impulses of their followers" (p. xi). To understand this tension, Condon's book has to be supplemented by David G. Bell's *Early Loyalist Saint John* (1983), which pays more attention to the rank and file. Condon considers these loyalists in the context of their opposition to the elite and their use of tactics previously used by the assemblies in the thirteen colonies. Missing, however, is an explanation of the experiences, disappointments, and dreams that led other loyalists to challenge the elite's blueprint for New Brunswick. With such a diverse group of people was there only one loyalist dream for New Brunswick?

JANICE POTTER
University of Saskatchewan

PHILLIP A. BUCKNER. *The Transition to Responsible Government: British Policy in British North America, 1815–1850.* (Contributions in Comparative Colonial Studies, number 17.) Westport, Conn.: Greenwood. 1985. Pp. viii, 358. $35.00.

Everyone interested in Anglo-Canadian relations will welcome the long-awaited publication of Phillip A. Buckner's study of British policy toward British North America in the eventful years of the rebellions and the advent of responsible government. His detailed account should help destroy the myths still cherished by many Canadians despite the writings of Helen Taft Manning: that British ministers were indifferent to Canada and unaware of the growing crisis during the 1830s until the rebellions suddenly caught them by surprise; that the British ministers were then rescued by Lord Durham, whose celebrated report borrowed the ideas of union from the English party in Lower Canada and the ideas of responsible government from the reformers of Upper Canada; and that under continued demands from the colonists the British government at last reluctantly conceded local self-government.

This was not, in fact, a process of devolution in which all the pressure for changes in the traditional but increasingly unworkable system of representative government and all the constructive proposals for reform originated on the North American side of the Atlantic. Buckner conclusively demonstrates that, particularly after the Whigs came to power in 1830, officials at the colonial department paid unremitting attention to the affairs of the Canadas, if only because internal Canadian dissensions might have adverse effects on the uncertain state of Anglo-American relations. Without the collaboration of local oligarchies, British policy makers were left to wrestle unaided with intractable problems of constitutional disharmony and rampant factionalism, exacerbated in Lower Canada by ethnic antagonisms given a sharper edge by the rise of an assertive French-Canadian nationalism. With a limited range of options and concessions available, officials in London tried to devise some means of satisfying colonial aspirations and restoring harmony without putting unbridled power into the hands of a French assembly, a situation, they feared, that might lead to civil war and American intervention. Had it not been for the French factor, the provincial legislatures probably would have received local self-government earlier from a British government prepared to disengage itself from the petty politics of the Canadian colonies. The rebellions in 1837–38 cleared the slate, and, once the French were seemingly submerged in the union of the Canadas in 1841, the way was open to a transfer of power to colonial executives increasingly controlled by dominant parties in assemblies. Tory ministers in London acquiesced in this declension of administrative power in the early 1840s, and their liberal successors openly acknowledged it.

In his analysis of this familiar story, Buckner does not depart in any significant respect from the views now commonplace among British imperial historians, but he has written the most thorough and authoritative account of this major crisis in Anglo-Canadian relations. Unfortunately, the publishers have done a disservice to reader and author by adopting an unpleasing form of reproduction from camera-ready copy with ragged right-hand margins, in contrast to the earlier, more attractively produced volumes in this series.

PETER BURROUGHS
Dalhousie University

ELINOR KYTE SENIOR. *Redcoats and Patriotes: The Rebellions in Lower Canada, 1837–38.* (Canada's Wings Illustrated Military History of Canada Series, number 1; Canadian War Museum Historical Publication, number 20). Stittsville, Ont.: Canada's Wings. 1985. Pp. 218. $29.95.

Elinor Kyte Senior has joined the ranks of Gérard Filteau, Aegidius Fauteux, Joseph Schull, and oth-

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ers who have produced works detailing the events of the rebellions of 1837–38 in Lower Canada (modern Quebec). Her precursors have typically paid more attention to the *patriotes*' activities than to those of the loyalists, but, in *Redcoats and* Patriotes, Senior pursues her interest in those forces supporting the established order that she displayed in *British Regulars in Montreal* (1981). In her latest study she outlines the loyalist response in Montreal to the rising agitation during the fall of 1837 and discusses at length the actions of loyalists as well as *patriotes* during the rebellions. This is not her only contribution, however. She reconstructs the ebb and flow of the major battles, providing useful maps of each. She points to the weaknesses of the *patriote* leadership and tries to rehabilitate the reputation of loyalist leaders. Loyalist commanders were not vindictive. On occasion their men, particularly the untrained volunteers, did get out of hand, but on other occasions the seeming excesses they perpetrated, such as the burning of homes, can best be seen as "disciplined military acts" (p. 190). Indeed, Sir John Colborne, "Old Firebrand" himself, evidently does not deserve the calumny heaped on him. He was an able, effective commander and recognized as such throughout the empire. Some will find such conclusions too generous.

Senior's concern is with the events of 1837–38, and her discussion of the causes of the rebellion is brief but interesting. She dismisses as unworthy of consideration the argument developed by *patriote* leader Louis-Joseph Papineau and later accepted by Filteau that the British deliberately forced a rebellion on the people. She can accept Fernand Ouellet's analysis of the ethnic and social character of the *patriote* leadership but doubts his contention that deep-rooted economic difficulties played a major role in precipitating the rebellions. Economic difficulties there were, to be sure, but, she rightly notes, the area north of Montreal, perhaps the most fervently prorebel region of the province in 1837, was largely unaffected by "serious economic dislocation" (p. 115). In fact, generally "the areas of disaffection in Lower Canada were the more opulent, populous, and fertile of the farming regions" (p. 5). Political grievances largely explain the rebellions.

Her concusions are similarly brief but provocative. Most in the province did not support the risings, although these had the effect of earning all the habitants new respect, for those who had fought had fought well. Nonetheless, the rebellions were unfortunate. Aside from the personal tragedies they engendered, they may well have delayed the achievement of responsible government—a sensible, if controversial, conclusion.

In any event, the causes and consequences of the rebellions are not the central concerns of Senior's book. The events of 1837–38 and their actors are, and here her analysis could be sharper in at least one respect. Her comments about the national and religious characteristics of the loyalists would be more convincing if buttressed by statistical evidence. Furthermore, her analysis of their mentality is suspect in at least one particular. Was it ever possible, as she suggests it was in the fall of 1837, that the Montreal Tories would have combined with the *patriotes* against the government? Probably not.

Whatever flaws it may have, and these are relatively minor, Senior's profusely illustrated book will prove to be of interest to the general and academic reader alike, providing, as it does, the best available account of the events of 1837–38 in Lower Canada.

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JEAN BARMAN. *Growing Up British in British Columbia: Boys in Private School.* Vancouver: University of British Columbia Press. 1984. Pp. 259. $29.95.

Growing Up British is a carefully researched study of private schools in British Columbia during the first half of this century. Jean Barman's thesis is that the relatively large number of British who immigrated to British Columbia during this period sought to replicate the upper-class way of life that they had enjoyed in England—particularly their positions of social leadership. Believing that private schools were important institutions in preserving that way of life, they founded a large number of these schools between 1900 and 1950, which had a profound influence on several generations of leadership. Since almost no records of these schools exist, the author conducted over one hundred fifty interviews with schoolmasters and students. She also made use of census data, newspapers, church records, and the like. The last third of the book contains an appendix of nineteen statistical tables on such subjects as school population by place of birth and occupations of graduates and their parents, a list of all the schools founded between 1860 and 1984, a full section of notes, an extensive bibliography, and an index. Finally, thirty-five illustrations give the reader a feel for these schools that could not be obtained from the written word alone. Some of the statistical tables are rather daunting: Number 19 is entitled "British Columbia Pupils Attending Vernon and Shawnigan Schools Following Paternal Occupation as a Proportion of All Fathers in That Occupation Whose Son's Occupation Is Known, Differentiated by Paternal Birthplace" (p. 186).

The author's presentation of her material is of uneven quality. Speaking generally, those sections of the book based on statistics are heavy going, while those based on interviews are interesting and often

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delightful reading. In chapter 2, "The Creation of Institutions," Barman details the founding of some forty schools in twenty-one pages; all but a handful were fly-by-night institutions that lasted only a short time. For these short-lived schools, the author needlessly includes in the main text the same information contained in the appendix. Had she concentrated on the founding of the most important schools and referred her readers to the appendix for the rest, chapter 2 would have been better balanced and more readable.

Chapter 5, "Realizing the Ideal," is, however, an admirable account of life in British Columbia schools during the period under discussion. According to one schoolmaster, a school should be based on the "7 Cs"—Christianity, classics, cricket, cadet corps, cold baths, courtesy, and corporal punishment. The major aim of all the schools was the development of character, a process that could best be achieved in a boarding school, away from the influence of parents who "permit late hours and general self-indulgence to an almost incredible degree" (p. 82). The chapter contains a detailed account of the daily routine of the boys, from the morning bell at seven, followed by a cold bath, until bedtime, which to one student was the greatest "pleasure" of the day: "retiring to my bed each night—where I could withdraw into myself, dream and *be alone*" (p. 88).

All the schools encouraged outdoor activities such as bird watching, collecting natural objects, and building "forts." Most of the schools emphasized things British, from speaking with a proper accent to following the Oxford-Cambridge boat race. By the same token eveything connected with the United States was anathema. The boys were taught to put women on a pedestal and warned against the temptations of sex. Punishments consisted of ridicule, detention, corporal punishment, and expulsion. Many of the boys were homesick and generally miserable at school, but many acknowledged that the experience had indeed been a preparation for life. Finally, there is the marvelous story of the boy who tied the doorknobs of two masters' doors together and then experienced some kind of schoolboy nirvana as he watched them tugging to get out (p. 103).

Space does not permit discussion of the other parts of the book: the study of important socioeconomic positions held by private school graduates in British Columbia; the portrait of Charles W. Lonsdale, headmaster of Shawnigan Lake School and the most dynamic figure in the book by far (*Time* did a piece on him in 1947); and the decision of the Canadian government in 1977 to subsidize private schools. In her concluding chapter the author states: "Education is without doubt a very powerful agent of cultural transfer. In the case of British Columbia, a handful of schools headed by men of conviction and commitment have had importance far beyond their numbers" (p. 173). In a sense the whole book is an effective brief in support of the statement.

FREDERICK S. ALLIS, JR.
EMERITUS
Phillips Academy

N. KEITH CLIFFORD. *The Resistance to Church Union in Canada, 1904–1939*. Vancouver: University of British Columbia Press. 1985. Pp. xii, 274. $24.95.

The title of this book is somewhat misleading, for N. Keith Clifford is concerned only with the Presbyterian minority that in 1925 stubbornly refused to join the Methodists, Congregationalists, and the Presbyterian majority in forming the new United Church of Canada. According to Clifford, at the core of this Presbyterian opposition to the United Church was to be found an urban-based, central Canadian conservative lay movement energized by "an affirmation of the denominational pattern of Canadian religious life and a defence of religious pluralism" (p. 241).

Clifford views the often-bitter battle between the two groups of Presbyterians largely through the research filter provided by the major anti-Union organization, the Presbyterian Church Association, whose records he has effectively used. Clifford's sympathies are obviously with this latter group, and his bias sometimes affects both the way in which he marshals his evidence and the way in which he reaches his conclusions. Clifford is particularly strong in tracing how the "antis" actually organized their campaign, especially in the period from 1904 to 1925. Considerable emphasis is placed on organizational problems and the rhetoric of opposition. Inadequate attention is devoted, however, to the possible motivations of the principal actors involved in the unfolding drama—people like the influential entrepreneur John Penman, who, despite the crucial role he played in the opposition movement, is a largely enigmatic and shadowy figure in this book. He and his supporters often seem to be lost in the rich texture of Clifford's descriptive narrative that, among other things, underplays the significance of situational pressures and theological differences as well as class, power, and possible psychological need.

Clifford is at his best in discussing how the provincial and federal governments attempted to deal with the myriad of problems arising out of the creation of the United Church. His final chapter, describing the means by which the Presbyterian minority regained its status in the late 1920s and

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1930s as the Presbyterian Church in Canada, is persuasively argued.

G. A. RAWLYK
Queen's University

CHARLES W. HUMPHRIES. *"Honest Enough to Be Bold": The Life and Times of Sir James Pliny Whitney*. (Ontario Historical Studies.) Toronto: University of Toronto Press. 1985. Pp. xii, 276. $18.50.

Political change comes slowly in Ontario, Canada's most populous province. In May 1985, forty-two consecutive years of Conservative party government came to an end with the victory of the provincial Liberal party. In 1905 James P. Whitney engineered a similar political watershed by becoming Ontario's first Conservative premier after thirty-four consecutive years of Liberal party rule. Charles W. Humphries's study of Whitney, the third biography to appear in the Ontario Historical Studies series, examines this formative period in Ontario history, an era marked by increasing urbanization and industrial development, educational and social reform, the opening up of Ontario's north, and the creation of North America's first state-run electrical utility, Ontario Hydro.

Despite the "life and times" subtitle, Humphries has written an unabashedly political biography. Relying extensively on the Whitney Papers, he provides us with a detailed and somewhat narrow focus on Whitney's years in opposition and his career as Ontario's premier between 1905 and 1914. Party politics and elections figure prominently throughout the text. Social, intellectual, and economic change in Ontario receives relatively short shrift. As his title implies, Humphries sees Whitney as a Progressive reformer. The premier's political career spanned an era of rapid urbanization, and Humphries cogently demonstrates that Whitney's political success lay in his ability to respond to the needs of an increasingly urban electorate, as well as in his willingness to downplay his party's legacy of anti-Catholicism and build bridges to the province's Catholic minority. During his years in office Whitney reduced electoral corruption, rationalized Ontario liquor laws while remaining an opponent of prohibition, provided permanent funding for the University of Toronto, and introduced boards and commissions to regulate public health, municipal growth, electric railways, workmen's compensation, and, most notably, the generation and sale of hydroelectricity. In this sense, Whitney's Progessive politics was analogous to the broader "search for order" that Robert Wiebe has analyzed for the United States during the pre–World War I era.

Whitney also avidly practiced what H. V. Nelles terms the "politics of development." In a province hungry for growth, Whitney and the Conservatives were not reluctant to use the powers of the state, particularly its crown ownership of natural resources, to promote aggressively the development of Ontario's northern forest and mineral frontier and its vast hydroelectric potential. Relying heavily on the work of Nelles and Christopher Armstrong on federalism and resource development in Ontario, Humphries treads on mostly familiar ground in this part of the biography. Unlike Nelles, though, Humphries sees Whitney's support for public ownership of hydroelectricity to be rooted more in personal conviction and a desire to sap the potential strength of socialism than in sheer pressure from Ontario manufacturers and municipalities.

This book fills an important gap in the still-undeveloped historiography of Ontario's political history. Humphries provides us with a well-written account of the origins of the Conservative party's overwhelming dominance of twentieth-century Ontario politics. Much work remains to be done, however, on the social, economic, and administrative history of Ontario during the Whitney era.

JAMES STRUTHERS
Trent University

MICHAEL D. BEHIELS. *Prelude to Quebec's Quiet Revolution: Liberalism versus Neo-Nationalism, 1945–1960*. Kingston, Ont.: McGill-Queen's University Press. 1985. Pp. xii, 366. Cloth $37.50, paper $14.95.

In the late 1940s and 1950s, what remained of traditional French-Canadian society and, particularly, the corrupt Union Nationale government led by Premier Maurice Duplessis, increasingly came under attack. In this book, Michael D. Behiels seeks to show that the ideological challenge to the Old Regime came from two competing movements. On the one hand, a group he calls the neonationalists, comprised of young intellectuals and journalists, set forth their views in newspapers and magazines like *Le Devoir* and *L'action nationale*. They were nationalists in that they sought means of improving the place of French-Canadians in the economy and in society, yet they parted ways with their conservative elders by attempting to come to terms with the largely urban and industrial society that Quebec had become. On the other hand, a decidedly smaller group of intellectuals, gravitating around *Cité libre*, a serious periodical with a tiny circulation, also wanted to modernize Quebec. This group, of which Pierre Elliott Trudeau was certainly the best-known spokesman, put forth liberal and social democratic values, not national ones.

The author seeks to show that in virtually all questions—educational reform, the role of organized labor, Quebec's place in Canada—the two

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groups were in conflict. That significant differences existed between them is true; still, on reading this well-researched synthesis, it does seem clear that the groups were often in agreement, at least ultimately. Whether one emphasized liberal democratic values or chose instead to give a prominent place to national values, reform of the educational system and a cleanup of the political scene appeared imperative.

The coverage that the author accords the *Cité libre* group can be questioned; after all, Trudeau and company obviously failed to appreciate the importance of national values within a minority group that feels itself a victim of flagrant inequalities and, indeed, that fears for its very survival. In Quebec, at least, the neonationalists won the contest and demonstrated that national values and liberal values were not necessarily incompatible.

The *Cité libre* group, of course, moved on to the federal scene, but their philosophy fell victim to Canadian realities. When Trudeau pushed for bilingualism, he was obviously recognizing the legitimacy of national and cultural values and admitting that French Canada was not getting a fair deal. When he jumped on the Canadian nationalists' bandwagon and put forward policies to lessen the American hold on the Canadian economy, he seemed to be admitting that his frustratingly rationalist critique of nationalism was no longer relevant.

Behiels presents an interesting analysis based on a wide variety of sources. His work constitutes a useful contribution to historians' knowledge of postwar Quebec, and his interpretation should provoke welcome new discussion of the years 1945 to 1960.

RICHARD JONES
Université Laval

LATIN AMERICA

TZVETAN TODOROV. *The Conquest of America: The Question of the Other*. Translated by RICHARD HOWARD. New York: Harper Colophon. 1984. Pp. x, 274. $6.95.

The Conquest of America is a study in the history of cross-cultural communication or the lack thereof. At issue is how individuals within the cultural-linguistic constraints of one society encounter and perceive those of another society. Recently, in *The Memory Palace of Matteo Ricci*, Jonathan Spence carefully documented how the sixteenth-century Italian Jesuit selected four images and four appropriate Chinese signs to address the Mandarins of late Ming China. Here, in the book under review, Tzvetan Todorov, the Bulgarian-French structuralist, deconstructs the sixteenth-century Spanish perception of the Aztecs (and to a lesser extent the Aztecs' perception of the Spaniards) as a history of the Other.

In four separate chapters Todorov uses a close reading of the pertinent primary sources to show four successive phases in a history of the Other: discovery, conquest, love, and knowledge. Columbus acted as a naturalist when he discovered America but not the Americans; lacking any appreciation of the Other's humanity, he instead stereotyped them as either "noble savages" or "dirty dogs." Because Montezuma relied on the Aztecs' signs of ritual discourse, he was prevented from coming to terms with the secular threat of the Spanish conquest, whereas Hernando Cortés successfully used the signs of Western typographic communication to manipulate and ultimately to overcome the Aztecs. The love the Spanish missionaries bore for the Aztecs was delimited by the Western debates on whether the natives were different or identical, equal or unequal to white human beings. And, finally, Western knowledge of the Aztecs was located along three axes, namely, value judgment of the Other, action of approaching or distancing the Other, and cognizance or ignorance of the Other's identity.

Far more than just a new, insightful piece of analysis, this book makes an important contribution to the methodology of intellectual history. In any discipline there is an epistemic connection between assumptions, method, and the content of knowledge. In our own time, when the nineteenth-century bourgeois assumptions underlying the genres of intellectual biography and history of ideas are no longer valid, the contemporary intellectual historian needs to ask what are the relations of the content of thought to the structures of language and of society. In other words, what constraints do language and society impose on thinkers and their thoughts? Along this line, Todorov's deconstructive history of the Other opens for us a new vista. This book is a "must" for all intellectual historians.

I have but one criticism to offer. Language is not an autonomous structure inside of which thinkers roam freely. Its constraints are motivated by and tied to existing power relations. With the same sixteenth-century structure, yet with different power relations vis-à-vis the Other (in the one case a less powerful Aztec, in the other a more powerful Chinese civilization), Cortés and Ricci confronted different problems, strategies, and prospects of knowledge. Their very different experiences reinforce my point that to Todorov's deconstructive method we must add Michel Foucault's injunction concerning the relation between knowledge and power: there is no knowledge without power, and no power without knowledge.

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ROBERT S. WEDDLE. *Spanish Sea: The Gulf of Mexico in North American Discovery, 1500–1685*. College Station: Texas A&M University Press. 1985. Pp. xvi, 457. $34.50.

This book is essentially a narrative history of the discovery, exploration, and settlement of lands washed by the Gulf of Mexico. Robert S. Weddle divides his work into four sections, each of which has both a chronological and a geographical base. "West from the Islands" opens with early Cuban history, Ponce de León in Florida, and early voyages to Mexico by Hernández de Córdoba and Juan de Grijalva; it ends with the conquest led by Cortés. In "Amazing Mexico" the region's conquest is followed from central New Spain, Francisco de Garay's voyage, and Cortés's response to it through the expansion southward to Honduras and northward to Nueva Galicia to the efforts to reach and conquer the Yucatán.

"The Continent of Florida" applies the geographic label broadly and examines the experiences of Cabeza de Vaca, Hernando de Soto, and Luis de Moscoso Alvarado; a Dominican expedition by Fray Luis Cáncer de Barbastro in 1549 and a second, equally disastrous one in 1554; other Spanish incursions into Florida; and the expedition led by Tristán de Luna y Arellano. In "The Building Rivalry" Weddle moves on to early English activities in the Gulf of Mexico, the Pedro Menéndez de Avilés explorations, the settlement of Nuevo León by Luis de Carvajal, late sixteenth- and seventeenth-century expeditions by the Spanish, incursions by the English, the French, and the Dutch, and the role of pirates in the Gulf region.

The author justifies his efforts in the concluding chapter, where he reinterprets the early history and cartography of the region from the sources. Weddle claims that too few modern studies have been written; of those that have been written, too many are "provincial" in outlook and use contemporary sources improperly. He believes that his efforts to look at the entire Gulf region and his reinterpretations of documentary evidence justify this study. The text lacks footnotes; each chapter ends with a section called "Sources and Notes," which vary from brief to lengthy and thorough but do not make it possible accurately to relate material in the text to the references; there are no direct citations.

That sixteenth- and seventeenth-century sources and maps are difficult to use is well known to scholars in the field. Although Weddle points out mistakes by earlier writers—some accidental and some owing to other authors' biases and misuse of evidence—the ideas presented in this book are no more convincing or conclusive. Specialists on any single aspect of what Weddle writes about will be disturbed by condensation of material, conclusions based on inadequate evidence, and errors in fact and interpretation. The book, which reads well, was designed to serve both the general reader and the specialist but really meets only the needs of the former. A careful reexamination of the sources and a reinterpretation of the early history of exploration and cartography of the Gulf of Mexico region clearly is needed. Weddle has provided a service by pointing out the need and by proposing many new interpretations, but the book does not fulfill its promise. It is a first step, but an inadequate one.

BRUCE B. SOLNICK
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J. BENEDICT WARREN. *The Conquest of Michoacán: The Spanish Domination of the Tarascan Kingdom in Western Mexico, 1521–1530*. Norman: University of Oklahoma Press. 1985. Pp. xv, 352. $27.50.

In a review of the Spanish version of this book, Robert S. Chamberlain wrote that the author "has added significantly to research studies on Spanish New World expansion" (*Hispanic American Historial Review* [August 1977]: 495). J. Benedict Warren's book has in the meantime become an indispensable base for any further work. The author takes the reader through sixteenth-century Michoacán like a host through his own house. The area was under the dominance of the Tarascan Indians when the Spaniards arrived. Adjacent to the west and north of what was then the Aztec empire, it included Lake Pàtzcuaro and the town of Tzintzuntzan, where the ruler, or *cazonci*, had his seat. To learn about the resources of this terrain that are relevant to the Spanish expectations of treasure, mineral wealth, and human and agricultural resources, we are made to follow the tracks of the conquerors.

Once we begin to accompany the first Spaniards on the scene in 1521–22, the haphazard relationship between actual conditions and the conquerors' understanding of them is carefully followed step by step. We are shown how the expedition of Cristòbal de Olid (1522) led to the Caravajal survey of Michoacán and the systematic granting of encomiendas. The book offers detailed information about early encomiendas, gold mining, slave and tribute labor, and Spanish and Indian mutual adaptation and maneuvering. In addition to many quoted texts, the present edition includes three appendixes of documents, also in translation.

Central to the book is the story of the last *cazonci*, an inexperienced young chief, from his first contact with the Spaniards to his execution in 1530. Warren's presentations of the intricacies of these early contacts will probably not be bettered in detail or judicious balance. Skillfully describing how the

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quick wealth of seized treasure gave way to the exploitation of labor, Warren also examines the drastic changes in Michoacán, which included the demographic decline resulting from war and disease, and the large movement of native people to suit the demands of miners, slavers, and encomenderos. The author meticulously chronicles various forms of Spanish economic enterprise in the region, especially the relationship between encomienda, estancia, and hacienda. From all this emerges a story of absentee ownership of encomiendas by Spaniards who lived in Mexico, which had such an important bearing on the political and economic aspects of the institution in the province.

The systematic evolution of more stable patterns in Michoacán had to wait for the arrival of the forceful episcopacy of Vasco de Quiroga in 1538. He continued the missionary effort begun by the Franciscans and, for nearly thirty years, exercised a humanitarian rule that brought him the title of founder of the Spanish province of Michoacán.

This book gives definitive answers to many questions about the events, people, and circumstances of the conquest. Progress will now have to be made by asking new questions.

URSULA LAMB
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JOHN TATE LANNING. *The Royal Protomedicato: The Regulation of the Medical Professions in the Spanish Empire*. Edited by JOHN JAY TEPASKE. Durham: Duke University Press. 1985. Pp. 485. $37.50.

The protomedicato was the Spanish peninsular institution charged with the examination and licensing of physicians and other medical practitioners. It was naturally extended to territories overseas where it had a checkered history, owing both to general difficulties of establishing effective jurisdictions in the vastness of the Spanish empire and to the peculiar situation of medicine there.

The primary feature conditioning the history of health care and hence of the protomedicato in the Spanish colonies was the chronic shortage of physicians. For example, that only one licensed physician served Mexico City in 1545 predetermined the entire pattern of health care in the capital: romance surgeons, algebrists (bonesetters), phlebotomists, and even priests moved in to compensate for the shortage of doctors. Since blood purity laws effectively kept nonwhites out of medicine, mulatto empirics (whether licensed as romance surgeons or not) filled the void in many cities, along with foreign doctors, who were technically not even allowed to migrate to the New World. Because it was impossible to control quality under such conditions, medicine became a low-prestige profession, its practice locked into a peculiarly vicious circle. Viceroys and cabildos were continually granting licenses to unexamined persons because of the unremitting demand for practitioners. This condemned the protomedicato to impotence.

Inasmuch as the protomedicato was funded on fees only (from examinations, licenses, and inspections of apothecary shops), nowhere was it able to exercise effective control over medical practice, and, in times of emergency such as epidemics, only the viceroy, not the protomedico, was able to mount an effective campaign of amelioration.

Although John Tate Lanning begins with a disclaimer to the effect that this study is institutional history and not a history of medicine, it is actually both. Lanning (who died in 1976, leaving the unfinished manuscript in the able hands of John TePaske) was a historian of ideas who was able to weave a rich tapestry with the social and intellectual threads. Thus, the chapter on obstetrics is particularly interesting because Lanning handles both the medical ideas (such as the revival of the cesarean section) and the social context well.

This is an important book, which will become the starting point for the future history of medicine in Spanish America. But it misses being a great book because too much space is devoted to repetitious institutional detail and broader linkages between medicine and society are underdeveloped. For example, the discussion of the great Peruvian medical school of the late eighteenth century is weak because of Lanning's incomplete awareness of the political role of physicians in the transition to independence, of the relationship between medical ideas and ideology (both Hipolito Unanue and J. M. Valdés participated in the "Dispute of the New World," which sought to defeat European claims of the Old World's biotic, climatic, and intellectual superiority), and of the social constitution of the group (he correctly identifies J. M. Dávalos as a mulatto but misses Valdés).

THOMAS F. GLICK
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RICHARD B. SHERIDAN. *Doctors and Slaves: A Medical and Demographic History of Slavery in the British West Indies, 1680–1834*. New York: Cambridge University Press. 1985. Pp. xxii, 420. $42.50.

After spending much of a distinguished career explaining and measuring the wealth derived by Europe from the Caribbean sugar-slavery nexus, Richard B. Sheridan now attempts to describe and explain "the opposite face of materialist success and riches—namely the catastrophic health and welfare costs of slavery and dependency, in short 'the curse of slavery and monoculture'" (p. xvii). More pre-

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cisely, he sets out to understand what it was about the sugar plantation system of the British West Indies that made it impossible for the slave population to achieve a natural increase, to assess the quality of health care provided, and to estimate to what degree, if any, this mitigated the harsh demographic equations.

It is a grand enterprise, compendiously written but achieving its effect more by accretion than by sharply focused and sequential argument. The book's plan is rather cumbersome, and the individual chapters are somewhat cluttered. Sheridan seems reluctant to discard any material, mixing contemporary with modern works and rarely making forthright judgments on either. Thus, it is difficult to distinguish quite what he adds to the sum of knowledge.

The first chapter, an uneasy mixture of general factors and particular illustrations, sets the overall tone. To a brief survey of the place of the West Indies in what he (surely erroneously) calls the South Atlantic system and an interesting summary of epidemiological implications, Sheridan appends lesser details, including a discussion of Alexander Tulloch and Henry Marshall's 1835 army medical report that is flawed by a paragraph of wrongly calculated statistics (pp. 15–16). He also discusses malaria and yellow fever in disproportionate detail, largely because other diseases, including smallpox (and its prevention), are considered later in an excellent chapter.

The bulk of the book, indeed, is divided between the general and the particular. Four central chapters provide a systematic if not very original summary of what is known about slave life styles, work patterns, treatment, health, and demography. The summary is weakened by Sheridan's concentration on sugar plantations to the virtual exclusion of other plantations and nonplantation slaves and, less culpably, by the completion of the book before the author could integrate the latest work published by Barry Higman and Kenneth Kiple.

Far more original are the other four chapters in which Sheridan presents his own discoveries about the white doctors employed in the African trade and the plantation colonies, about slave hospitals, and about the details of European medical practice in the British West Indies. Because of his often uncritical recital of contemporary diagnoses and treatments, Sheridan steers close at times to the white doctors' overly generous self-evaluation; for example, he provides a table (p. 307) intending to show the care and attention accorded to sick slaves on Harmony Hall Estate, Jamaica, in 1824, which to a modern eye simply demonstrates that the treatment was probably useless where not actively harmful. Elsewhere, though, Sheridan is less kind to his Scots doctors, especially the many whose training was sketchy or who persisted in the fatuities of humoral theory against all evidence. He also includes an important chapter that gives overdue credit to the white doctors' black assistants and even more bravely attempts to assess the contribution made by the informal practitioners of Afro-Caribbean folk medicine in the slaves' own quarters.

The concluding chapter, moreover, usefully ties together many loose ends in summarizing and evaluating the net results. Sheridan lists, without convincingly ordering, the factors that prevented a natural increase of the slaves, pointing out that these were mostly beyond the control or understanding of the planters and the doctors they employed. Yet the excess of deaths over births did decline as slavery progressed, and some of the improvement can be credited to the growing number of doctors who saw the value of improved hygiene, isolation, rest, and better food for the sick; the value of smallpox inoculation and vaccination and a few local medicines; and the baneful effects of heroic doses of medicines made from mercury, arsenic, antimony, and opium.

That the West Indian black population did quite suddenly increase once slavery ended, especially where the former slaves were able to escape the sugar plantations, was, as Sheridan tellingly concludes, the ultimate indictment of the slave sugar plantation system. That the improved demographic performance was accompanied by the flight of most of the white doctors should caution against an exaggerated view of their efficacy. For such conclusions, as much as for the encyclopedic material deployed throughout, this book is a valuable addition to the slavery literature.

MICHAEL CRATON
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ANNE PÉROTIN-DUMON. *Être patriote sous les tropiques: La Guadeloupe, la colonisation et la révolution, 1789–1794*. (Bibliothèque d'Histoire Antillaise, number 10.) Basse-Terre: Société d'Histoire de la Guadeloupe. 1985. Pp. 339.

Students of French Caribbean history will welcome Anne Pérotin-Dumon's book as an excellent addition to the limited number of works devoted to Guadeloupe. Some students have been concerned that this area has been treated most often in a less than scholarly manner. Pérotin-Dumon has avoided this pitfall and has produced a keenly perceptive book that has been thoroughly researched and reflects the best of scholarship. The framework of the book brings into focus most of the colonial period and ends with a concluding statement about what it means to be a patriot and to act patriotically in the tropics.

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The book is divided into three major sections: the colonial, the revolutionary, and the republican periods. In the third section, the excellent conclusion supports and describes the title of the work. In an unusual manner, the author includes a number of documents and a comparative chronology of events in Guadeloupe, Europe, and some portions of America. In essence, this chronology states, on facing pages, what is happening year by year in Guadeloupe and Europe; a special index of persons is also included. These features represent a new approach to writing on the French Caribbean. An excellent bibliography of books and documents highlights the latter part of the work.

In part 1 Pérotin-Dumon discusses colonization, focusing on the colonial system, the immigration of French to Guadeloupe, and the implantation of the sugar and slave culture. Of special interest is the author's description of the emergence of Creole society, along with city and rural life. The second part takes the reader through the period of the French Revolution and its effect on commerce and deliberations in the colonial assembly. Interestingly, the examples of Martinique, the Royal Commission, and other institutions imposed on Guadeloupe at this time are treated in only a cursory fashion. The revolts that took place in the Windward Islands are given especially short shrift.

The essence of patriotism is best shown in part 3, "The Colonial Republic." Here one is more inclined to look at the comparative notes toward the end of the book. Although Pérotin-Dumon's discussion is designed, in part, to give some comparative insights on Guadeloupe and what was transpiring in France, she does not do this in a well-focused manner. Revolutionary events led to local political turmoil and left a residue of opponents who disagreed about proper procedures and ideology. Defections in the colony from both sides of the revolutionary cause were a daily occurrence, and one is reminded of Whigs and Tories during the American Revolution.

The influence of the French Revolution on French thinking was mixed. Despite emphasis on the rights of man, for example, little was said or done about slavery during the revolutionary period, and it appears that the slave revolt in Saint Domingue (Haiti) did not influence Guadeloupe as one might expect. At this point, however, England did everything possible to isolate Haiti and what was occurring there. The French Revolution did give rise to social and political changes. As some writers have said, Guadeloupe did not experience the discontinuity that other areas did, but with political change came the need for new political alignments. Freed blacks and *petite blancs* had to make some kind of common cause since they existed in almost equal numbers in the population. The social and economic changes created tensions, although not the kind that would break the feasibility of coexistence. What was and was not patriotic continues to be relevant, because Guadeloupe is still today an overseas department of France.

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DAVID NICHOLLS. *Haiti in Caribbean Context: Ethnicity, Economy, and Revolt*. New York: St. Martin's. 1985. Pp. x, 282. $27.50.

David Nicholls is a noted scholar and friend of Haiti. One of his recent works, *From Dessalines to Duvalier* (1979), stands as a significant contribution to the historiography of that island republic. Scholars, however, will be disappointed with Nicholls's most recent work, *Haiti in Caribbean Context*. Despite his claim to have blended some of his previously published articles with new material to produce a coherent monograph, I remain unconvinced. Often he penetrates but fails to develop interesting subjects. His treatment of the migration of ethnic minorities into and out of the Caribbean is a case in point. Nicholls says much about the movement of Haitians and Levantine settlements but little about anything else.

My biggest complaint, however, is the author's failure to provide the "Caribbean context" that he promises in the title of his book. He discusses Haitian culture well but seldom compares it with the rest of the Caribbean, or even with the French West Indies. He deals at length with the language conflict within Haiti but says nothing about French Creole (Kréyol) anywhere else. Could comparisons be made with Haitian voodoo? What about Santeria in Cuba? And, when the author speaks of Caribbean revolts, he largely restricts himself to three nineteenth-century uprisings in Haiti. What about Jamaica in 1865 and other important West Indian revolts?

Nicholls's work has some strengths, however. He demonstrates that the *cacos* and *piquets* who became enmeshed in nineteenth-century Haitian politics were not a rural proletariat but a rustic middle class. The *ton-ton macoutes* of François "Papa Doc" Duvalier's regime bear a strong resemblance to these two groups. Duvalier, in fact, relied heavily on the black middle class for support. Nicholls further indicates why power shifted so neatly from Papa Doc to his son Jean-Claude. But Jean-Claude is dangerously eroding his father's power base and is increasingly dependent on the support of the elite.

My parting criticism of Nicholls's work concerns his editorializing. Editorial comments often pass in the historian's craft as "interpretation," but Nicholls goes too far. He obviously sees little good in impe-

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rialism, foreign aid, Third World dependence on developed nations, or Christian missionary efforts. These historical trends, however, have a bright as well as a sordid side. What about the efforts of the Anglican bishop C. Alfred Voegeli to promote Haitian culture and a similar effort by the Methodist missionary Duvon Corbitt in Cuba?

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RAÚL GARCÍA HERAS. *Automotores norteamericanos, caminos y modernización urbana en la Argentina, 1918–1939*. Buenos Aires: Libros de Hispanoamérica. 1985. Pp. 141.

Few countries were more closely tied economically to Great Britain in the late nineteenth and early twentieth centuries than Argentina. In the period between the two world wars, however, companies based in the United States began to make significant inroads into the Argentine market. Leading the way were automobile manufacturers—notably, Chrysler, Ford, and General Motors—that during the interwar period came to dominate this new and increasingly popular mode of transportation.

This work traces the introduction of North American automobiles into Argentina and the consequences both for the host country and for its principal trading partner, Great Britain. Focusing on what he calls "the interimperialist rivalry between the United States and Great Britain" during these years (p. 7), Raúl García Heras details the often-complex responses of the main actors to these developments. For the British the main concern was to retain their virtual monopoly over various forms of public transportation in the face of North American competition in the private transport sector. For the Argentines the trick was to encourage alternatives to the British monopoly without unduly alienating the nation on which much of their economic livelihood was ultimately dependent.

García Heras, drawing on a substantial body of Argentine and British documentation, tells this story of "interimperialist" rivalry in a solid and sophisticated fashion. In contrast with many other Argentine treatments of similar issues, this study is straightforward and refreshingly free of dogmatic assertions and emotional polemics. It does, however, suffer from a lack of documentation from U.S. sources, and, although the introduction of U.S. automobiles and the Argentine government's efforts to build roads to accommodate them receive considerable attention, the relationship of these factors to urban modernization is not a well-developed theme. In sum, however, this is a useful and welcome contribution to Argentine economic history.

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LAWRENCE E. HARRISON. *Underdevelopment is a State of Mind: The Latin American Case*. Lanham, Md.: University Press of America or the Center for International Affairs, Harvard University, Cambridge. 1985. Pp. xviii, 192. $17.95.

Lawrence E. Harrison, a twenty-year veteran of the U.S. Agency for International Development, originally believed, as did other early Alliance for Progress enthusiasts, that "Latin America was in trouble principally because of U.S. neglect" (p. xv). Later he decided, and wrote this book to argue, that culture, more than any other factor, explains "why some countries develop more rapidly and equitably than others" (p. xvi). Comparing countries that have had relative success with those nations whose record has been poorer, Harrison contrasts Costa Rica with Nicaragua, the Dominican Republic and Barbados with Haiti, Australia with Argentina. He tries to demonstrate that specifically Hispanic cultural factors have delayed "democracy, economic progress, and increased social justice" (p. 148). Harrison rejects dependency theory as "both fundamentally flawed and a major impediment to progress in Latin America" (p. 151) and suggests reforms in Latin American leadership, religion, education, the media, and management to accelerate "constructive cultural change" (p. 176).

Harrison traverses a large territory. He writes that the book "does not present conclusive evidence for the validity of the thesis" (p. xviii); rather, he intends it to be a spur to further research and action. Aware of the difficulties that his version of the often-useful culturological thesis faces, he asserts his acceptance of multifactoral determination of development and claims that his interpretation is neither racist nor a form of "Yankee boosterism" (p. 167).

Just how useful this book can be is questionable, for the presentation is seriously flawed, largely because of its cultural reductionism and Harrison's lack of clarity in using categories of analysis. The comparative accounts include non-Hispanic societies. The author gives practically no attention to Brazil, yet he generalizes for Latin America. He has not developed cases for countries like Colombia and Mexico. The cultural analysis is weakened by the employment of large and barely manageable categories, from the ethos of political leadership to the bringing up of children, as well as by inferences about assumed relationships and outcomes.

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This method strikes home when Harrison cites Tomás Roberto Fillol's 1961 study to show how child rearing and a particular form of indulgent individualism are related and produce, in Fillol's native Argentina, autocratic fathers and obedient sons and "a high need for dependency and aggression" (p. 118). One wonders precisely the degree to which this, in relation to other factors, is valid in explaining Argentine political instability, slow growth, and tolerance for social injustice. Harrison might also have considered, for example, the cultural force of nationalism, a sentiment of considerable strength in revolutionary and reactionary regimes alike, but this would not have yielded a sufficient explanation.

Harrison also neglects other significant matters that bear on the question of equitable development. Among these are the powerful demonstration effect, which has produced both enormous expectations and bitter disappointment, and population increases, which helped minimize potential benefits of economic growth that occurred in much of Latin America in the 1960s and 1970s.

Harrison seems to deny that effective modernization is simply Americanization or that it must follow precisely the examples of Barbados or Trinidad, yet these views, which Harrison's refutation requires the reader to consider, are the most provocative part of this unsatisfying book. In the absence of a major cultural transformation, can there indeed be rapid, equitable, democratic, and relatively autonomous development in Latin America? That, to paraphrase Hamlet, is the question.

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JOHN C. SUPER and THOMAS C. WRIGHT, editors. *Food, Politics, and Society in Latin America.* (Latin American Studies.) Lincoln: University of Nebraska Press. 1985.

Most scholarly discussions of food in Latin America focus on issues of agricultural production and trade. John C. Super and Thomas C. Wright, the editors of *Food, Politics, and Society in Latin America*, have brought together a series of essays to consider not only the production but also the distribution and consumption of food. Their concern is to explore the social, cultural, and political structures that shape production practices and nutritional regimes throughout the region. Thus, essays consider such topics as historical continuities and changes in production, distribution, and consumption of food; the conflict between producers and consumers of food; and the impact of public policies on national and international food systems. Editors and contributors argue that, although food has occasionally been a scarce resource in Latin America, its availability is primarily determined not by geophysical limitations on production but by class structures and public policies that influence what is produced and who has access to various types of food. The essays in the volume are disparate in specific focus but find thematic unity in exploring such influences on the food system. Contributing authors represent the disciplines of history, political science, agricultural economics, anthropology, and rural sociology.

The first essay, written by Super, considers the changes that occurred in food production and consumption when Old World plants and animals were introduced with the conquest of Latin America in the sixteenth century. Wright, in a broad essay on urban distribution systems, demonstrates the important role food has played in the historical development of political consciousness among the urban poor in general and organized labor in particular. Vincent C. Peloso, in a case study of nineteenth-century Peru, adds considerable force to the argument that it is the political process of allocation, not absolute levels of production, that determines diet and availability of food for various social classes. This essay is complemented by Nancy Forster and Howard Handelman's study of revolutionary Cuba, in which they indicate that nutritional regimes have improved despite continued poor performance of the agricultural sector. International aid and trade figure centrally both in a chapter by Lana L. Hall on the differential impact of United States PL480 food aid on Colombia and Brazil and in another chapter, contributed by Eleanor Witte Wright, on a long history of public policy efforts to respond to food trade dependency in Venezuela. Public policy is also central to a discussion by Rose Spaulding on food and agriculture in Nicaragua after 1979 and to a critical analysis of the adoption of the technology of the green revolution for agricultural production presented by Ivan Sergio Freire de Sousa, Edward Gerald Singer, and William L. Flinn. A complementary essay by Roland Bergman argues that subsistence agriculture is an efficient form of production for many smallholders in Latin America. Methodologies for estimating and interpreting data on agricultural production and population are considered in a chapter by James W. Wilkie and Manuel Moreno-Ibanez.

This book can be recommended for its thematic concern with the complex factors that determine national and international food systems and for its historical and multidisciplinary focus. The volume reflects a growing awareness among policy makers, planners, and analysts that agricultural and rural development policies need to be framed within a

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conception of an overall food system encompassing production, distribution, and consumption. The editors have undertaken a difficult task, and the result of their effort is perhaps more suggestive than definitive. This volume should therefore stimulate concrete and intensive research into how production, distribution, and consumption have been altered over time by changing technology, class structures, political regimes, and public policies.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

CLAUDE A. BUSS, editor. *National Security Interests in the Pacific Basin*. Foreword by W. GLENN CAMPBELL. Stanford, Calif.: Hoover Institution Press. 1985. Pp. xxii, 317. $27.95.

CLAUDE A. BUSS, Introduction: Great Power Confrontation. R. L. J. LONG, The Pacific Theater: Key to Global Stability. B. R. INMAN, Great Power Confrontation in the Pacific Basin. EDWARD TELLER, National Defense in the Pacific Basin. PAUL WOLFOWITZ, The Interests and Policies of the United States. THOMAS HAYWARD and G. J. PRICE, Discussion. CLAUDE A. BUSS, Introduction: Northeast Asia. RODGER SWEARINGEN, Security Implications of Siberia and the Soviet Far East. YOICHI MASUZOE, Japan's Defense Posture: Toward Closer Cooperation with the United States. KOO YOUNGNOK, The National Interests of the Republic of Korea. HAN SUNG-JOO, The Republic of Korea and the Major Powers. YASUO TAKEYAMA *et al.*, Discussion. CLAUDE A. BUSS, Introduction: Southwest Pacific and the Indian Ocean. HENRY S. ALBINSKI, Australia, New Zealand, and the U.S. Security Interests. T.B. MILLAR, Australia and the Security of the Pacific Basin. DESMOND BALL, U.S. Installations in Australia: Agenda for the Future. RICHARD KENNAWAY, The Interests and Policies of New Zealand. CLAUDE A. BUSS, Introduction: Southeast Asia. DOUGLAS PIKE, The Security Situation in Indochina. LIE TEK TJENG, The Power Balance as Seen from Jakarta: A Projection for the 1980s and Beyond. CHANDRAN JESHURUN, The Interests and Policies of Malaysia: A Study in Historical Change. OBAID UL HAQ, National Security in the Pacific Basin: A View from Singapore. SUKHUMBAND PARIBATRA, Thailand's Interests and Policies. SALVADOR P. LOPEZ, The Politics of Philippine Security. GUY PAUKER *et al.*, Discussion. CLAUDE A. BUSS, Introduction: China. JONATHAN D. POLLACK, China's Role in Pacific Basin Security. TUN-HWA KO, The Interests and Policies of the Republic of China. SHIRLEY W. Y. KUO, Economic Development and Security: Perceptions and Policies of the Republic of China. YUNG WEI, The Republic of China and the Pacific Basin: Policy Perspectives in the 1980s. RALPH H. CLOUGH, Recent Trends in Chinese Foreign Policy.

C. B. MACPHERSON. *The Rise and Fall of Economic Justice and Other Papers*. New York: Oxford University Press. 1985. Pp. 154. $19.95.

The Rise and Fall of Economic Justice. Problems of Human Rights in the Late Twentieth Century. The Prospects of Economic and Industrial Democracy. Liberalism as Trade-offs. Do We Need a Theory of State? Human Rights as Property Rights. Property as Means or End. Pluralism, Individualism, and Participation. The Economic Penetration of Political Theory. Democracy, Utopian and Scientific. Hobbes's Political Economy. Hampsher-Monk's Levellers.

NELSON H. MINNICH *et al.*, editors. *Studies in Catholic History in Honor of John Tracy Ellis*. Wilmington, Del.: Michael Glazier. 1985 Pp. xxi, 765. $35.00.

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REVIEWS OF BOOKS

TO THE EDITOR:

Dragos Kostich's review of the late George Soulis's *The Serbs and Byzantium during the Reign of Tsar Stephen Dušan (1331–1355) and His Successors* (*AHR*, 90 [1985]:918–19) requires at least three remarks.

First, Kostich's contention that "conquest as a prime motive and goal can be attributed at this time only to the Ottoman protagonist" seems rather peculiar if one bears in mind that in Dušan's time the Ottomans had hardly started conquering the Balkans, while Serbia between the 1280s and Dušan's death in 1355 had expanded enormously through constant conquests.

Second, it is not quite correct to say that "John Cantacuzenus brought the Turks across the Dardanelles to fight as mercenaries against the Serbs." Cantacuzenus's main use for the Turkish auxiliaries was to fight against John V Palaiologos and his supporters, Cantacuzenus's opponents in the Byzantine civil war. Only secondarily were the Ottomans used against the Serbs (see G. Ostrogorsky, *History of the Byzantine State*, Oxford, 1968: 517–18).

Third and most important, Kostich's last paragraph contains a series of incorrect statements concerning the battle on the river Maritsa. Serbia was *not* "joined by its western neighbors, Bosnia and Hungary, in 1364" for that battle, and the battle itself took place in 1371, *not* in 1364. King Louis of Hungary could hardly have "barely managed to escape" from the battlefield, because he was *not* there in the first place; only the Serbs, led by King Vukašin and his brother Uglješa Mrnjavčević fought and lost the battle and their lives. Finally, there was *no* "second Maritsa River confrontation" at which the "Serbian magnates, acting as independent allies" were defeated.

Kostich was misled because, apparently, he relied on S. Shaw's *History of the Ottoman Empire and Modern Turkey* (vol. 1, Cambridge, 1976: 118–19), which contains numerous errors of fact, instead of using more reliable works (e.g. Ostrogorsky, *o.c.*; *Istorija Srba*, vol. 1, ed. S. Ćirković, Belgrade, 1981; R. Mihaljčić, *Kraj srpskog carstva*, Belgrade, 1975).

BARIŠA KREKIĆ
University of California, Los Angeles

TO THE EDITOR:

Dragos Kostich's reference to Ottoman rule in Southeastern Europe as being "a cover of darkness, stagnation and oppression" (*AHR*, 90 [1985]: 918–19) is a rather startling renewal of medieval Christian religious and modern Balkan nationalist prejudices against Muslims in general and Turks in particular which I had thought had been largely dispelled by numerous scholarly publications on the Ottoman Empire since World War II. The Ottoman conquest of the area was a product of decay and degeneration which had taken place in the Byzantine Empire and its successor states before the Ottomans came on the scene. The Turks conquered a Constantinople which had long since been despoiled and depopulated by the Latin Crusaders as well as by centuries of internal strife and dissension. The Ottomans brought vitality and renewal to the Balkans for some three hundred years before their Empire became subject to decline as well as to the inevitable results of nationalist aspirations for independence on the part of all the subject peoples, including the Turks. Ottoman rule in the Balkans, far from being the "oppression" characterized by Kostich, rather was characterized by tolerance of different ethnic, social, and religious communities to a considerably greater extent than any other major

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state or empire in Europe. What Christian state in Europe allowed those of other religions to live and worship freely and to prosper economically as did the Ottomans in the *millet* system? Ask the thousands of Jews who flocked into the Ottoman Empire from persecution in Spain, Central Europe, and Russia. The dark and systematic persecution, massacre, and expulsion of millions of Turks and other Muslims in the Balkan states which gained their independence from the Ottomans during the nineteenth century contrasted sharply with the toleration which they had experienced in Ottoman society. Dr. Kostich and those who may still accept his viewpoint should consult the brilliant collection of articles published by Benjamin Braude and Bernard Lewis in *Christians and Jews in the Ottoman Empire: The Functioning of a Plural Society* (New York, 1982).

STANFORD J. SHAW
University of California, Los Angeles

PROFESSOR KOSTICH REPLIES:

In my review of Soulis's work a summary of events in the four decades after Dušan's death became inaccurate as it was reduced to a concluding paragraph. I accept the responsibility and the opportunity to clarify the issues.

In view of significant differences in interpretation of events, and even chronology, in the many studies of fourteenth-century Southeastern Europe, a consultation of a broad spectrum of sources is required. Professor Krekić's sense of certainty might be modified by consulting seminal works, such as K. Jireček's *Istorija Srba* (Belgrade, 1952) and S. Novaković's *Srbi i Turci* (Introduction by S. Ćirković, Belgrade, 1960). *Istorija naroda Jugoslavije* (B. Grafenauer *et al.*, eds., Belgrade, 1953), D. Obolensky's *The Byzantine Commonwealth* (New York, 1971), and J. Barker's *Manuel II Palaeologus* (New Brunswick, 1969) contain valuable information and a variety of viewpoints which may prevent oversimplification. There is, in regard to sources, an unusual awkwardness on these pages as one of my two critics (Krekić) challenges the accuracy and veracity of the other (Shaw). While disagreeing with the views in Professor Shaw's letter, I find no grounds for dismissing his research into Turkish sources.

Turkish campaigns in the Balkans since the early 1300s are thoroughly discussed by Novaković (pp. 47–121). After the Ottoman crossing of the Dardanelles in 1353, Cantacuzenus enlisted them against John V, "who had established himself in Enos at the Maritza estuary, within reach of Serbian borders" (Novaković, p. 135); and John V's forces along with the Serbs were defeated in that year as Dimotika. Quite obviously John V, alone, was not Cantacuzenus's prime concern.

The fighting in the Maritsa River area in the 1360s was complex before, and even after the transfer of the Ottoman capital from Bursa in Asia Minor to Adrianople in the mid-1360s, Dr. Shaw's reference to a clash in 1364 cannot be lightly dismissed. The Maritsa River Battle of September 1371—the "sirpsindiği," or the Destruction of the Serbs—probably was fought at the present site of the town Sirpsindiği, about fifteen miles northwest of Adrianople. Although as Krekić rightly notes, it involved only the southern magnates on the Serbian side, their defeat resulted in Ottoman suzerainty in Serbia's southern territories and led to the defeat of Serbia and its allies at Kosovo in June 1389. While King Louis confronted the Turks in 1366 in Bulgaria, his actions were directed mainly against Serbia, which he invaded unsuccessfully in 1353 and again in 1359 after he obtained the designation as "Military Leader Against the Serbian Schismatics" (Jireček, vol. 1, p. 239). The chances of success of King Sigismund's Crusade were reduced after Serbia's defeat in 1389 and resulted in the disaster at Nikopol in 1396.

Serbian expansion and the Ottoman conquest of Byzantine provinces can be seen as identical processes only through gross oversimplification. Dušan's consistent policy was to preserve the Byzantine administrative, legal, economic, social, and cultural systems in the provinces he acquired, and he viewed himself as a successor rather than conqueror of Byzantium (Novaković, chapters 3–5; Jireček, vol. 1, part 4, chapters 1–3). Both Cantacuzenus and Demetrius Cydones noted the similarities between the Greeks and the Serbs (Obolensky, p. 257), and at least one pro-Serbian party in Thessalonike shared this sense of continuity. The Ottoman conquest, on the other hand, was a part of a Holy War, the *jihad* against the infidel. The Ottoman *millet* system, as Professor Shaw notes, provided certain advantages for the conquered, but it was a matter of convenience rather than enlightened tolerance. An empire founded upon religious law, which could not apply to a vast majority of its European inhabitants, found it expedient to delegate limited authority in civil law to non-Islamic ecclesiastic institutions. While basic cultural and national sense of continuity was preserved within the *millet*, art and architecture came to a halt in the conquered nations, systematized education was eradicated—not until the late 1880s was the first Serbian school authorized in the Ottoman provinces—and regression rather than stagnation prevailed in social development. Collectors of the *blood tax* removed periodic quotas of young boys, slave trade continued into the nineteenth century, and all opposition was countered by slow death by impalement or mutilation. Religious

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conversion conferred full citizenship and privileges, and it was available to all. That only relatively very few took advantage of this opportunity speaks eloquently of prevailing views of the Ottoman state. A consideration of some of the features which are facts of history is not any "renewal of medieval Christian and modern Balkan nationalist prejudices."

DRAGOS D. KOSTICH
Long Island University

TO THE EDITOR:

I must say I was a little surprised that Michael Hurst's splenetic diatribe against Roger Casement was printed in your publication (*AHR*, 90 [1985]: 936). There were so many objectionable things in it the mind boggles.

One thing especially stuck out. Hurst is cavalier in his bandying about of the word "traitor." Whom did Casement betray? Surely one can only betray his own country, and Casement was an Irishman, not an Englishman. I doubt the average Irishman regards Casement as a traitor.

Hurst, among other things, questions Casement's sanity. He brings up homosexuality as if that were a clear indication of mental disorder. He also appears to feel that anyone who questioned the benevolence of British rule and tried to struggle up from under the jackboot of English tyranny was a lunatic. By that reasoning Parnell, Gandhi, and Jefferson were all madmen.

Hurst dismisses Casement's Irish nationalism as vanity. Perhaps it was, partly. But it is a queer sort of vanity that leads to a hangman's noose. Casement may have been an ineffective patriot, but he was willing to give his life to cast the English out. Hurst cannot seem to understand that and sneers. His sneers are not something to be desired in any self-respecting historian.

HOPE LEMAN
Corvallis, Oregon

PROFESSOR HURST REPLIES:

The points raised in the Leman letter can be dealt with quite succinctly. Casement was a traitor to his state—the United Kingdom of Great Britain and Ireland. England and Ireland are both countries in it, but at the time of Casement's treason were not legally units to which political loyalty could in jurisprudence be owed. Not only was Casement a United Kingdom subject. He was someone long one of its civil servants—above all in causes connected with destroying gross injustices on foreign soil the like of which had been extirpated from areas of British jurisdiction. And in any case, the behavior of both the overwhelming number of Irish Nationalist politicians and of the Irish prisoners of war he tried (unsuccessfully) to suborn does suggest his conduct was in fact regarded as treasonable by a huge proportion of his own Catholic countrymen (let alone the Protestant).

Casement's homosexuality has to be regarded as important because it was of an ungoverned nature. An identical condition in a heterosexual would also suggest serious imbalance of character. Were our letter writer to go so far as to read the book I reviewed, she might well even agree that, taken as a whole, the cumulative impression left by Casement's overall conduct was one pointing to conditions of marked abnormality. Which means, of course, that the doctor, not the hangman, should have been his ultimate earthly guardian.

It is difficult to understand why tyranny if it cropped up on British territories should necessarily have been or be English. And to assume tyranny of any sort (English, Irish, Scots, or Welsh) obtained in Ireland at the point Casement first embraced his particular form of Irish nationalism would be grievously unscientific. No one of any professional moment would attempt to gainsay the fact that before the aftermath of the 1916 rebellion the vast majority of nationalist Irish were Home Rulers—and Home Rule meant Ireland's continuing inclusion in the United Kingdom. Parnell was a Home Rule leader. Unlike Gandhi and Jefferson, he did not wish to end overall London sovereignty. Perhaps also our correspondent ought to be reminded that Gandhi supported the British Empire during most of both world wars. Not for him any expedition behind enemy lines with the aim of recruiting Indian prisoners for action under Japanese command. Nor was he killed by "English tyranny"—a fanatical Hindu did the deed in the same spirit as that behind the murders of Collins and O'Higgins in the Irish Free State by the IRA. Jefferson was certainly guilty of treason in the eyes of London but got away without punishment because of Yorktown. Yet all this is beside the point, as I never suggested opposition to United Kingdom rule was necessarily a sign of even a mild mental disorder, let alone madness. Though today the condition of many formerly imperial territories might well suggest that many agitators lacked realism and foresight.

That such a letter as Hope Leman's could today be written is proof of how much in history teaching the bland lead the bland, not making clear that Ireland after 1800 was not legally a British colony but a part of the metropolitan country with one hundred or more representatives in the House of Commons (not to mention thirty-two in that of the Lords); not bringing out the difference between a country and a state; and failing to differentiate between the different degrees of opposition to un-

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welcome government. Naturally the less perceptive do not appreciate the sarcasm of the perceptive. That does not, however, constitute sufficient reason for banning it or dubbing it unfair. Let our protester read some Gibbon. Then she will feel the scorpion's lash. Like me he was a Magdalen rather than a maudlin man.

MICHAEL HURST
St. John's College
Oxford University

TO THE EDITOR:

In his letter published in your issue of December 1985 (p. 1328), your reviewer, Robert Olson, has retracted his allegation that I "endorsed, praised, and aided in the publication" of Joan Peters's *From Time Immemorial*. But he clearly cannot bring himself to do so without qualification. He resorts to feeble sophistries by saying that the author's Acknowledgments page shows that I aided in her research and that this is tantamount to aid in publication. Leaving this peculiar logic aside, what the Acknowledgments page says is that I directed Miss Peters to the location of archives in Britain. This is in fact what precisely happened on the one occasion that I met her, when she applied to me for information about British archives relevant to her research. Such information one usually provides to enquirers as a matter of course, without scrutiny of motive. It is disagreeable that this should be made the pretext for dragging my name into polemics and quarrels with which I desire to have nothing to do.

ELIE KEDOURIE
London School of Economics

ERRATUM

Because of a computer error, several lines were dropped from the third paragraph of John T. O'Connor's review of Helmut Kötting's *Die Ormée, 1651–1653: Gestaltende Kräfte und Personenverbindungen der Bordelaiser Fronde* (*AHR*, 90 [1985]: 1208). The third paragraph should read:

After a succinct overview of the historiography of the *Ormée*, Kötting focuses on the city of Bordeaux—its role in the French economy, the relationship between local elites and royal administrators, the loyalties and power of each group in the population, the worlds of Catholics and Huguenots, and a summary of past unrest and rebellion in the area, especially since the sixteenth century. He next reviews the divisions that rent the city as the Fronde progressed from one stage to the next, its tangle of hatreds, fears, and ambitions fueling the last act of the rebellion.

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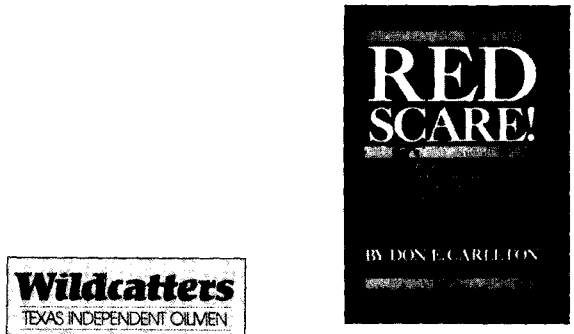
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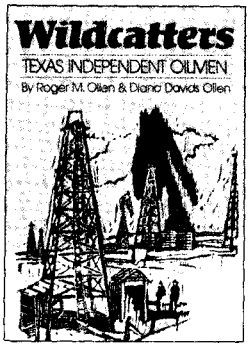
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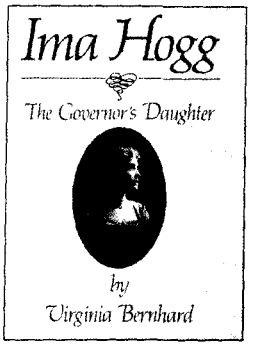
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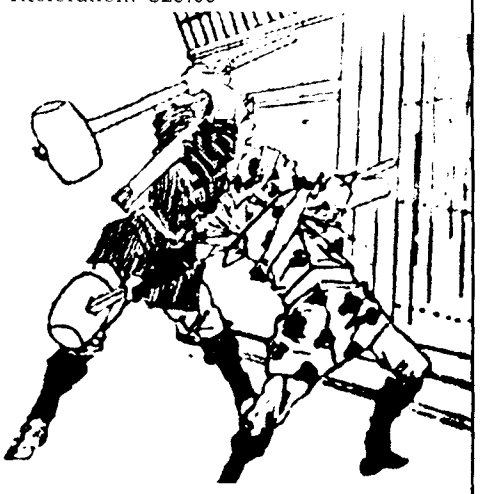
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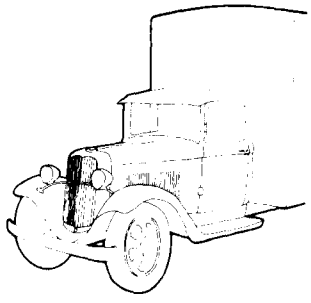
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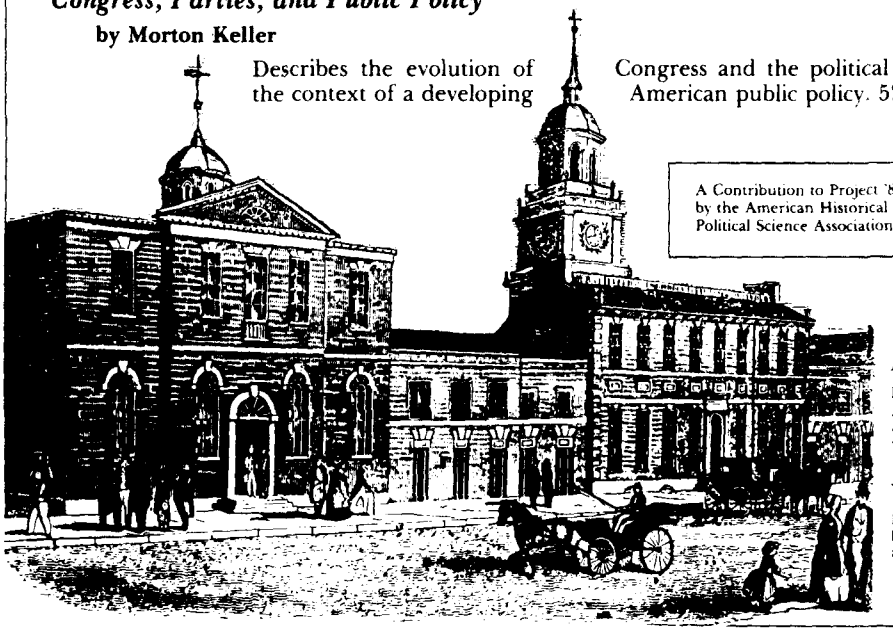
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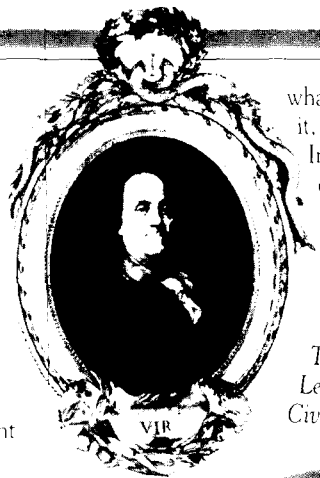
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